

EMERGING EDENS: THE BIOTECHNOLOGY TRANSFORMATION OF AMERICAN
AGRICULTURE

-By

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A DISSERTATION PRESENTED TO THE GRADUATE SCHOOL
OF THE UNIVERSITY OF FLORIDA IN PARTIAL FULFILLMENT
OF THE REQUIREMENTS FOR THE DEGREE OF
DOCTOR OF PHILOSOPHY

UNIVERSITY OF FLORIDA

1997

This dissertation is dedicated to the memory of
my father, Jerome, who died before he could see it,
but not before he could encourage me in its birth
and its completion.

ACKNOWLEDGEMENTS

The author wishes to express his appreciation to his committee of advisors: Dr. Frederick Gregory, Dr. Robert Hatch, Dr. Betram Wyatt-Brown and Dr. Chuang Liu, but most especially to his major advisor, Dr. V. Betty Smocovitis for her support and friendship. Special thanks also goes to Dr. Loukas Arvanitis for his encouragement.

The author would like to thank Christopher Koehler, Gary Weisel and Michael Futch for acting as a valuable cohort and friends during this second run at graduate school. A special thanks goes to Richard Pizzi for his support, encouragement and friendship through some hard times.

The author is most grateful to his family, especially to his father, Jerome, now deceased, and his mother, Stephanie, who provided without condition or fail the love and encouragement needed to make it through this time of career readjustment. Words cannot express the author's indebtedness, love and gratitude to these two splendid people. The author would also like to thank his brother, Michael, and his sister-in-law, Maryalice, for their love and support.

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Abstract of Dissertation Presented to the Graduate School
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May, 1997

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Major Department: History

Biotechnology in American agriculture developed in a context of institutional, state and national politics, as well as of scientific change. It "emerged" in a framework of private and public ideological crises that lent meaning to biotechnology as a unique solution to a catastrophic loss of faith in the inherited mythos of American agriculture, and a disillusionment with the idea of scientific progress itself. The rhetoric of "science in farming" was coupled to the belief that agricultural research (basic and applied) was a national priority, an idea that had been honed from the eighteenth through the twentieth centuries.

Agricultural chemistry came under attack after World War II, in part from the development of the modern environmental movement and the ecological mentality, both scientific and popular. At the same time overproduction in

the U.S. led many to question the role of productivity research. From one perspective this can be seen as a crisis in which failure of old science and technologies led to demands for a new paradigm.

By the early 1970s, the solution to these problems "emerged" in what many saw as a growing biotechnology revolution and was pursued vigorously at many universities. By the early 1980s, it became a key issue in the National Association of State Universities and Land Grant Colleges. Having embraced the promises of tissue culture, scientists and administrators found it easy to recognize the promise of molecular genetics. They charted a new course for land grant research. Plant genetic engineering was, in fact, ancillary to the shift in technological direction. Prior technologies and a firm belief in the new agricultural chemistry--that of DNA and the unity of life under the macromolecular rubric and the evolutionary synthesis--had already transformed the rhetoric and reality of research. The old paradigm was re-invigorated. The threatening new paradigm ("holistic" farming and an abandonment of USDA's production mentality) was defeated or absorbed.

CHAPTER 1 INTRODUCTION

Biotechnology, Agriculture and Revolutions

The Emergence of Emerging

Historians have recognized the mythic power of America as the New Eden. From the beginning of colonization, America has been the fabled land of plenty -- food and fiber in abundance, the lure to a Europe on the continual edge of want.¹ Politically, from Washington and Jefferson to the Populists, from the Homestead Act to modern calls for the preservation of the family farm, the agrarian component of this idyllic mythos has been a key facet in the ideology of American republicanism. The United States envisions itself as a nation of yeoman farmers, feeding their families and feeding the world.²

¹ R. D. Hurt in his American Agriculture: A Brief History (Ames: Iowa University Press, 1994) states: "In the colonies . . . relatively cheap and easy access to land gave American farmers freedom to produce for themselves and the marketplace and leave subsistence farming behind within a relatively short time after settlement." (p. 69). This freedom and cheap land was such that "Many colonial farmers agreed that America was the "best poor man's country in the world."" (p. 37).

² Leo Marx in The Machine in the Garden: Technology and the Pastoral Ideal in America (New York: Oxford University Press, 1964) comments on an aspect of this, what he calls,

America has also been the quintessential home of the Enlightenment ideal of progress. From the first, that progress has been enshrined in rational, scientific thought and the technology it created. The necessity of that progress has been a cultural given since America was born of a Europe that had embraced progress nearly unequivocally.³ Wedded to the American mythos of renewal, abundance and rebirth in the land and frontier, it has provided a vision of the primary methodology by which to achieve the promised agricultural Eden.⁴

"sentimentality" towards the rural life in America: "We see it in our politics... in the power of the farm bloc in Congress, in the special economic favor shown to "farming" through government subsidies, and in state electoral systems that allow the rural population to retain a share of political power grossly out of proportion to its size." (p.5). Although Marx was writing in 1964, I would argue that little has changed, consider the issue of tobacco subsidies in today's politics.

³ Clive Ponting on p.150 of A Green History of the World (New York: St. Martin's Press, 1992) states that: "The eighteenth century was marked by a wave of optimism about the future and the inevitability of progress in every field." He quotes the English writer William Godwin who stated in 1793 that: "Three fourths of the habitable globe are now uncultivated. The improvements to be made in cultivation, and the augmentations the earth is capable of receiving in the article of productiveness cannot, as yet, be reduced to any limits of calculation." America was a significant portion of that three fourths and represented much of that potential.

⁴ It is telling that in 1863, in his first report, Isaac Newton, Commissioner of Agriculture of the newly established United States Department of Agriculture, not only extols agriculture as the source of civilized morals and society but blames the fall of Rome on her repudiation of agrarian values. Newton then delineates the line of

Progress gained an inevitability of direction, a Lamarckian "power of life" striving toward an ultimate perfection that could be aided (by the wise) or resisted (by the foolish).⁵ New possibilities "emerged" from this forward

modern agricultural progress, pointing out Great Britain's debt "in a large measure to Lord Bacon for her early attention to progressive agriculture. That great thinker gave to the world inductive philosophy, which teaches man to experiment, to question and test nature by her great alphabet of soils, gases, elements and phenomena - a philosophy which is at once positive, progressive and eternal, making man the "minister and interpreter of nature."" Newton points out that in the United States "great and manifold progress has been made in agriculture." (p. 9). "It is common to see the best plough, rollers, cultivators, reapers, threshers, fanners, hay and cotton presses, sugar mills, horse and steam powers, and a thousand other labor-saving machines, the results of skill and science. This imperfect sketch of agricultural improvement in England and the United States is given in order to show that progress [italicized in the original] has not been the result of mere routine farming, but of practical applied science [italicized in the original] - of classified knowledge." (p. 10).

⁵ Richard Burkhardt in The Spirit of the System: Lamarck and Evolutionary Biology, (Cambridge: Harvard University Press, 1977), points out that not only was Lamarck's idea of evolution a theory very much belonging to the eighteenth century, but that his "model of organic change took into account both the "natural" progress of organic development and the modification of this progress by constraining circumstances (p.145)." It involved a natural form of progress. "Lamarck identified the primary factor of organic change to be a natural tendency toward increased complexity, which he attributed to "the power of life" (p.151)." Such a belief appears to underlie the belief in the evolution of technological progress and the concept of emergence. It may help to explain why the attempt to remove constraining circumstances in the forward development of technology seems to be such a primary goal for many of technology's most ardent defenders. As will be demonstrated, the agricultural biotechnologists saw the technology as emergent, progressive and capable of being assisted or thwarted, but not subject to any form of social construction in its creation and forward movement. The politically useful

movement, possibilities seen as somehow beyond the control or predictability of mortal men, who were at best expected to embrace and use the new potentials to which technology gave birth. The embrace of science was the means to that great end.

An equally American idea has been that of taming the frontier, subduing Nature, "civilizing" the land. God himself in Biblical genesis set forth this proscription of dominion and control as man's role in the old Eden -- how much more necessary in the new promised land?⁶ Although this image of conquest was primarily seen as a paean to the courage of the American male (the New Adam), the role of agriculture and technology in his success was recognized. Technological marvels emerging from the march of progress were tools to be used for dominion over the Earth to the benefit of humanity.⁷

to their goals, it seemed part of their tacit ideology as well, framing their abilities to think about technology as much as it directed their political aims.

⁶ In Nature and the American (Berkeley: University of California Press, 1957), pp. 1-13, Hans Huth details how Americans exalted the pioneers and their conquest of nature.

⁷ In his first report, Isaac Newton refers to the American farmer as one who "... has no master -- no favors to beg of man. He has a sturdy independence of character, adorned perhaps, by culture and refinement. He belongs to a class of citizens who hold in their hands five-sixths of the wealth of the country and its entire political power; and the hands which have wrought this wealth are able to defend the Constitution which makes us one people." (p. 14). But also, the farmer requires more to be perfected: "In order to

As for today, it is a truism that we are in the midst of a biotechnology revolution that somehow emerged in the 1970s and confronted us with its power. Historical accounts of biotechnology have accepted the revolution with little equivocation. Attempts have been made to determine when and where the revolution started, who were or were not the "founding fathers" and whether there was a true meaning differentiating "old" biotechnologies such as plant breeding and zymotechnology from the "new" and "revolutionary" ones such as genetic engineering.⁸

make the farmer most successful, and thus to advance agriculture, and the great interest of the republic, he should study chemistry as applied to soils, plants, grains, animals, manures, climates, localities and tillage. . . . There is no occupation so intimately blended with all the natural sciences; to which geology, chemistry, botany and entomology are such valuable auxiliaries. Of all human pursuits, agriculture is first in order, in necessity, and importance. The best farmer is always the most intelligent man, and a community of knowledge is one of the strongest ties that can bind and bless society. The simple argument, therefore, is this: increased scientific and practical knowledge increases man's power in a tenfold ratio; agricultural knowledge therefore, begets productiveness [*italics in original*], and in the same proportion develops the wealth, the prosperity and the progress of our country." (p 17).

⁸ In the introduction to his book, Robert Bud discusses the extensive history of the word biotechnology from its coining in 1917 Russia. He outlines the concepts, practices and people associated with its modern development from early zymotechnology (brewing, cheesemaking, breadmaking--anything to do with fermentation) to cell culture and genetic engineering. He details what he sees as "The emergence of biotechnology from zymotechnology . . . "and in the philosophies of a vitalistic technology. . . ." These he sees fused with "engineering conceptions of a biological technology within enduring institutional forms in the United States. . . ." See: R. Bud, The Uses of Life: A History of

This dissertation will argue, however, that from the point of view of any biotechnology "revolution" in American agriculture, such an analysis is too simplistic. By ignoring the conceptual paradigm on which agricultural biotechnology, we miss the essentially conservative, in fact non-revolutionary nature of this new technology which enhanced, rather than replaced, an earlier scientific world view. We also miss the historical background of the more vociferous debates, past and current, concerning this new technology.⁹

Biotechnology, especially agricultural biotechnology,

Biotechnology (Cambridge: Cambridge University Press, 1993), p.5. Bud follows a global perspective in his history of biotechnology, concentrating as much or more on Europe than the United States. His "institutional forms" do not include any details on USDA or the SAES system. Rather, he concentrates on individual universities such as the Massachusetts Institute of Technology and government bodies such as the National Institutes of Health.

⁹ Although this dissertation will often discuss the issues in terms of "revolutions," crises and paradigms, it is not seeking to embrace a strictly Kuhnian perspective. In his The Structure of Scientific Revolutions (Chicago: University of Chicago Press, 1970) Thomas Kuhn argues that science proceeds from incommensurable paradigm to paradigm through intervening crisis and revolution. The crisis in agriculture seems part of a broader cultural war between concepts of progress and science and various forms of post-modern technological malaise coupled to cultural and environmental movements extremely complex in their origins. The debate was not between scientific paradigm A and scientific paradigm B but rather between the self-defined forces of science and anti-science (though for some it was only "anti" a particular kind of "progressive" technology-driven science). Thus the conflict falls out of an easy placement in Kuhnian terms. This dissertation sees the development of biotechnology as evolutionary, not revolutionary -- but also as due to a social form of natural selection, not the Lamarckian "emergence" that most of its advocates and critics seem to have envisioned.

did not "emerge" as some startling and unexpected discovery, but rather was the stepwise outgrowth of research dedicated, in advance, to its creation. The methodology of "recombinant DNA"¹⁰ was a pleasant surprise in its facility. But genetic engineering was the goal of a host of earlier laboratory techniques and approaches that paved the way for the ultimate adoption and acceptance of recombinant DNA as the logical next step.¹¹

Numerous researchers have commented extensively on the wide-scale effects of this perceived emergence of biotechnology on government, university and industry in both

¹⁰ Recombinant DNA is the technology through which DNA (deoxyribonucleic acid) fragments can be excised enzymatically from one organism/species and spliced into the DNA from another, thereby creating a new combination -- an artificial hybrid of both gene sources. This allows laboratory transfer of specific, individual genes at the same time as eliminating the need for sexual compatibility between the sources for the transfer. Thus recombinant DNA is the primary tool in modern genetic engineering practices.

¹¹ R. L. Phillips in "Genetic Engineering of Plants: Some Perspectives on the Conference, the Present, and the Future," pp. 453-466 In: Kosuge, Meredith and Hollaender (eds) Genetic Engineering of Plants: An Agricultural Perspective (New York: Plenum Press, 1983) summarizes the situation: "What is plant genetic engineering? There seems to be some confusion about the definition of plant genetic engineering. Some people deliberately exclude tissue culture and wish to consider only the isolation, introduction, and expression of foreign DNA in a plant cell. . . . A broader definition would emphasize that the focus is at the cell level and involves the interfacing of all aspects of cell and tissue culture, molecular biology and gene transfer. This definition reflects the enormous scope of the effort and the many research activities that have to be intertwined in order to apply such basic information in an agricultural context (p.454)."

medicine and agriculture.¹² This dissertation offers, instead, a preliminary examination of the effects of the biotechnology "revolution" on agricultural research from a narrower historical view. It argues that the very concept of "emergence" was a rhetorical tool used to validate the new technology and to divorce it from old research methods. These previous technologies were universally seen as sources of both social and scientific problems for agriculture in the post-1950s world.

The mythology of progress implied that the new was both inevitable and better. This was critical to the claim that biotechnology "emerged." In addition, emergence conveyed an evolutionary naturalness which presented society with something unplanned for to which it must respond. An analogous technological parallel was the emergence of atomic energy. Bombs and nuclear power were portrayed as natural outgrowths of physics, inherent in the nature of the atom

¹² A complete review would not be possible. Significant recent works specifically dealing with agriculture include: J. R. Kloppenburg, First the Seed: The Political Economy of Plant Biotechnology (1492-2000) (New Haven: Yale University Press, 1988), which has an excellent general discussion of privatization and the movement of the land grant universities into biotechnology; L. Bush, W. B. Lacy, J. Burkhardt, and L. R. Lacy, Plants, Power and Profit: Social, Economic, and Ethical Consequences of the New Biotechnologies (Cambridge: Basil Blackwell, Inc., 1991) is also exemplary in its perspective on the broader issues in these areas. R. Bud, The Uses of Life: A History of Biotechnology (Cambridge: Cambridge University Press, 1993) provides an historical perspective from zymotechnology to the 1980s.

itself. Society is responsible, not for initial development, but only for dealing with the ramifications and adapting to the "emergent" objects of technology once they are revealed as facts of nature.¹³

Biotechnology, at least in agriculture, and one might argue in biology as a whole, did not "emerge," nor was it "discovered." It was created with the open-eyed pursuit of a reductionist path that would turn life to genetics, genetics to chemistry and chemistry to chemical engineering -- goals at the heart and birth of United States Department of Agriculture and the land-grant university enterprise. Its rapid adoption was due, in part, to biotechnology's political utility, its ability to be a solution to problems that confronted agricultural research unexpectedly at the height of its post-WWII triumph. The technology itself did not emerge, but was an inevitable and logical manifestation

¹³ Glenn T. Seaborg, a Nobel Prize winning physicist and nuclear power advocate is quoted by Edward Cornish, The Study of the Future (Washington: World Future Society, 1977). "Humanity is rapidly approaching a series of crises that can be managed only through some radical departures in man's dealing between with the relationship between energy and matter. Nuclear energy holds one key - a crucial one - to the successful resolution of these crises. . . . we will be able to move all mankind ahead into a new era of human advancement. . . ." Seaborg says: "we must learn to live with the atom wisely. This means we must recognize, anticipate, and deal with all the environmental aspects and prospects of nuclear energy (p. 141)." This is similar to the perceived requirements to embrace biotechnology and learn to live with it as an emerging technology. There is no suggestion that there is any choice in the matter of the technology's development or existence.

of the scientific views and practices upon which it was based.

The mechanistic and reductionistic paradigm has been best described by one of the pioneers of molecular biology, Francois Jacob. In 1973, although apparently unaware at the time of the new recombinant DNA technology that was developing, he wrote: "The aim of modern biology is to interpret the organism by the structure of its constituent molecules. In this sense, modern biology belongs to the new age of mechanism."¹⁴ That this matches with the aims of the National Association of State Universities and Land Grant Colleges (NASULGC) Committee on Biotechnology is apparent from their description of the relevance of biotechnology for agriculture: "The new biotechnology research capabilities... provide a new basis for changing plant and animal productivity on the basis of the directed modification of specified genes and gene systems (i.e. "single-gene genetics") rather than the more heterogeneous approach of "whole organism genetics" which may result in compromising the desired qualities."¹⁵ Single genes become the

¹⁴ F. Jacob The Logic of Life: A History of Heredity (Princeton: Princeton University Press, 1993). p.9.

¹⁵ The National Association of State Universities and Land Grant Colleges was formed in the 1880s to deal with agricultural issues. Its Committee on Biotechnology published Emerging Biotechnologies in Agriculture: Progress Report II. Here (p. 22) the emphasis is on "new" biotechnologies and the belief that it is the reductionism

constituent molecules by which organisms are to be manipulated and understood.

This dissertation treats a relatively brief period from the late 1960s to the mid 1980s.¹⁶ The advent of biotechnology at the University of Florida is used as a focal point for examining, through a single land-grant university (LGU), how and why the LGU system changed. Not only was the change at the University of Florida representative of other such universities, but it had repercussions on the national level due to the strong influence Florida had on the nation's developing agricultural biotechnology policy.¹⁷

of genetics to a controllable, single gene or gene system level that is the source of the new technology's inherent power.

¹⁶ The majority of primary sources used in this work were obtained from the University of Florida archives and the Institute of Food and Agricultural Resources archives in Gainesville, the Congressional Record, the NASULGC Committee on Biotechnology records (copies held currently in the office of Judy Kite, IFAS, Gainesville), as well as miscellaneous newsletters and the "proceedings" of various scientific conferences held at the time.

¹⁷ The question may be asked: Why Florida? Most people do not think of Florida and agriculture in the same breath, except for orange juice. Agriculture is little remarked compared to Disney World, space shuttles, and Miami beaches. But Florida was and remains one of the top three agricultural states in the nation with one of the widest diversities of field and horticultural crops, oranges being simply the tip of the agricultural iceberg. Then, as now, Florida was a key player in tropical agriculture concerns and dominated much of the Southern Agricultural Experiment Station Region agenda. In addition, at this critical juncture in the early 1980s, the University of Florida

Between 1977 and 1985, Francis Aloysius "Al" Wood was Dean for Research at the University of Florida (UF), Institute for Food and Agricultural Research (IFAS). He was responsible for research and program planning for one of the largest State Agricultural Experiment Station (SAES) systems, and became the first UF Director of Biotechnology. Wood was important on the national level for the broad-based transformation he helped initiate through his role in the National Association of State Universities and Land-Grant Colleges and as one of the key research directors in the SAES System. At the state level, his leadership set Florida on a course that, by 1991, would make it the largest spender in biotechnology among all the land grant universities.¹⁸ As such, he is an exemplar of the scientists and administrators in the agricultural research community of the period.

The 1970s were a time of transition in US agriculture, in large part due to increasing social attacks. Almost all reviewers of the subject have cited several key influences on the agricultural system at this time. For example, in 1973 the Hightower Report: Hard Tomatoes/Hard Times

dominated key positions related to biotechnology in the National Association of State Universities and Land Grant Colleges (NASULGC) hierarchy.

¹⁸ Committee on Biotechnology, Division of Agriculture, NASULGC, 1992, Emerging Biotechnologies in Agriculture: Progress Report X. p. 26.

lambasted California agricultural scientists for betraying the small-farmer and migrant labor to agribusiness.¹⁹ Simultaneously the fruits of "Silent Spring" were ripening and the traditional pesticides that had formed the basis of the post-World War II technological boom were defeated (as with DDT) or under severe threat. There were entirely new regulatory schedules and a new agency, the Environmental Protection Agency (EPA), for agriculture research to deal with and a strong push for change by civilian-activists in the way agriculture was done, especially in the light of the new ecology movements.²⁰

By the late 1970s, in the developing nations, the so-called Green Revolution, appeared to founder with the rising prices of OPEC oil embargoes.²¹ In the United States that same revolution produced grain surpluses that threatened the

¹⁹ J. Hightower, Hard Tomatoes, Hard Times (Cambridge: Schenkman Publishing Company, 1973).

²⁰ L. J. Lear, Bombshell in Beltsville: USDA and the Challenge of "Silent Spring." Agricultural History 66 (2): 151-170. 1992.

²¹ Jack Doyle points out how the increased prices of the fertilizers necessary for the green revolution varieties proved devastating for the poorest of farmers and a boom for agribusiness. J. Doyle, Altered Harvest: Agriculture, Genetics and the Fate of the World's Food Supply (New York: Viking Penguin, Inc., 1985) pp.261-270. He points out that "As the debate over the effectiveness of the Green Revolution continues, biotechnology and genetic engineering are being touted for their agricultural potential in developing countries." p. 269.

family farm with a drastic loss of revenue, exacerbating the effects of increased production costs.²²

Not only was practice under attack on the farm front, but in the ivory towers of the agricultural research enterprise as well. The land-grant colleges and the SAES had suffered severe blows and a fall in public confidence. They were blamed for these "failed" technologies that produced overproduction at a harsh environmental price, and also held to ridicule in successive government reports. The agricultural research and education system was cited as a haven for applied agricultural "hacks" who had compromised its ability to conduct basic research and training.²³

Perhaps equally important, the very possibilities of the old scientific approaches were seen to be in danger. Numerous researchers pointed towards an increasingly asymptotic approach to "fixed" productivity levels in major crop species, leading to fears of ultimately diminishing returns in any increasing application of current

²² S. Wittwer, pp. 315-317. In L. Busch and W. B. Lacy (eds) The Agricultural Scientific Enterprise: A System in Transition (Boulder and London: Westview Pres, 1986).

²³ For a description and bibliography of these complaints against agriculture see: D. B. Schweikert and J. T. Bonnen, "Policy Conflicts in Agricultural Research," pp. 13-27 In L. Busch and W. B. Lacy (eds) The Agricultural Scientific Enterprise: A System in Transition (Boulder and London: Westview Pres, 1986).

technologies.²⁴

The world of agriculture, then, was suffering a crisis of confidence. On both the farm and in the land grant university, one could sense self-doubt in the scientific, political, private and public spheres. However, by 1977, when Al Wood arrived at the University of Florida as Dean for Research, the grand "solution" to these problems was becoming apparent. Wood and others in the agricultural community embraced the promise of the new molecular biology. They urged adoption of biotechnology approaches as the best hope for the SAES and all agricultural research. The biotechnology revolution was overtly recognized as the solution to all of the enumerated problems that had led to the crisis.

It is important to assess the driving forces for this seemingly sudden shift in the highly traditional agricultural enterprise to an apparently vastly different technological approach. To do this it is necessary to understand something of its scientific premises, especially as the agricultural community, including researchers and administrators such as Wood, saw and interpreted the situation at the time. Without this perspective, the "faith" aspects of the so-called revolution will be hidden behind

²⁴ USDA., Economics, Statistics and Cooperative Services, Agricultural Productivity: Expanding the Limits, Agricultural Information Bulletin 431, Washington D. C. 1979.

the rhetoric and no firm understanding of the motivation for the dramatic changes will be possible.

By the late 1970s Genentech, the first "star" among the biotechnology entrepreneurial companies on Wall Street, had demonstrated the feasibility of taking an animal gene, interferon, and inserting it into a bacterium.²⁵ The technical mechanisms for splicing and cloning DNA were thus in place. This process of transformation was as yet unfeasible for higher plants and animals, so the search was on for methodologies of transformation and biological vectors to carry DNA into the more complicated plant and animal cells.

The first indications that the plant pathogen, Agrobacterium tumefaciens actually inserted a portion of its own genes into the infected host were only just being proposed (and hotly debated) in the late 1970's. Plant DNA viruses, through the early 1980s in fact, seemed to offer as much hope at this time for serving as a vector for genetic engineering.²⁶

Plant science did have one advantage that animal

²⁵ Robert Teitelman, Gene Dreams: Wall Street, Academia, and the Rise of Biotechnology (New York: Basic Books, Inc. Publishers, 1989).

²⁶ R. C. Garder, "Plant Viral Vectors: CAMV as an Experimental Tool" pp. 121-142, In: Kosuge, Meredith and Hollaender (eds) Genetic Engineering of Plants: An Agricultural Perspective (New York: Plenum Press, 1983)

science lacked -- regenerating cell cultures and protoplast technology. Individual plant cells from a number of species could be separated, induced to grow in culture medium for mass production and used to grow vast quantities of "clonally" related plants. Additionally, cells from different species of plants could be fused to regenerate hybrid offspring incapable of being produced in the "natural" world.²⁷

To the very end Al Wood would trumpet these as fully legitimate biotechnological tools (somewhat against the developing wisdom that equated biotechnology with genetic engineering). He saw them as the means by which the molecular biology revolution in plants would be accomplished. This tendency to maintain the connection between "older" biotechnologies and the new, even to the point of considering plant breeding as a form of biotechnology, was a typical example of the agricultural communities' insistence on the continuity of the agricultural enterprise. It exemplifies the non-revolutionary nature of this new revolution.²⁸

²⁷ D. Evans and C. E. Flick, "Protoplast Fusion: Agricultural Applications of Somatic Hybrid Plants," pp. 271-288, In: Kosuge, Meredith and Hollaender (eds) Genetic Engineering of Plants: An Agricultural Perspective (New York: Plenum Press, 1983)

²⁸ In almost every report or interview Al Wood gave on biotechnology, he stressed the value of protoplast fusion and cell culture. This continuity is repeatedly stressed by

Of more significance to agriculture, then, was the economic and legal framework in which the new technology took shape. This was what made it a promise of salvation, financial salvation at least, for the struggling land-grant agriculture system. This was what offered political hopes to the battered USDA, struggling not only against scientific assault but against presidential administration relatively unfriendly to agricultural interests.²⁹

The Plant Variety Protection Act allowed USDA to issue certificates of protection for sexually reproduced plants. But this act did not protect clonal plants and was not as strong a protection as patent rights.³⁰ Not until 1980 was the ability to claim patent ownership of life affirmed by the Supreme Court in *Diamond v. Chakrabarty*.³¹

Equally, if not more, important, Congress finally

USDA and the NASULGC and in the very nature of the concept of old v. "new" biotechnologies.

²⁹ The Reagan administration persuaded Congress to reduce agricultural spending in order to help reduce the budgetary deficit. Congress did so over the objections of all agricultural groups with the exception of the Farm Bureau. R. Hurt, American Agriculture: A Brief History, p. 356.

³⁰ The issue of plant patenting and its commercial and research implications are ably discussed in H. C. Kloppenburg's First the Seed (Cambridge: Cambridge University Press, 1988) pp. 130-155.

³¹ R. Teitelman, Gene Dreams: Wall Street, Academia and the Rise of Biotechnology (New York: Basic Books, Inc. Publishers, 1989).

allowed land grant universities to gain financially from patents on discoveries made within their jurisdiction. The promise of an even more profitable interaction with industry was born.³² The response to this in Florida, under Al Wood's direction, was not merely an example of a nation-wide trend, it was also part of the background to the development of biotechnology throughout the land-grant system.

By 1982 word had gotten out sufficiently to all the land-grant institutions. They realized it was finally time to make a strong assessment and to take charge before someone else did. In April of that year the Division of Agriculture of the NASULGC established its first Biotechnology Committee, chaired by Al Wood. Frequently present in a continuing guest status was John Fulkerson -- Principle Scientist of the Cooperative State Research Service of USDA.³³

To these ends in fall, 1983, the second annual NASULGC biotechnology report, the so-called "Silver Bullet," was released. Titled Emerging Biotechnologies in Agriculture: Issues and Policies, Progress Report II, this black-spined, silver colored document was the compilation of a select

³² S. Wittwer, pp. 315-317. In L. Busch and W. B. Lacy (eds) The Agricultural Scientific Enterprise: A System in Transition (Boulder and London: Westview Pres, 1986).

³³ Committee on Biotechnology, Emerging Biotechnologies in Agriculture: Issues and Policies, Progress Reports I-VII, 1982-1988.

committee of deans and directors of various land-grant universities and agricultural experiment stations across the nation. This opening shot in the war for agricultural biotechnology autonomy included prescriptions for the development of new institutions and new competitive grants specifically for biotechnology in agriculture. The entire document was a manifesto of what could be done, what needed to be done and the goals which the biotechnology committee intended to lobby for. The report represented an historical nexus and symbol for the entire shift in attitude to the fighting stance of research agriculture.³⁴ The "Silver Bullet" and the subsequent reports of the NASULGC biotechnology committee had the desired effect of helping to promote and tailor a change which the research directors saw as necessary, desirable, and, in some ways, inevitable.

Partly because of its position statements, perhaps more because of the survey data that was the first and most extensive of its kind on the current and planned state of biotechnology in the SAES, it was quoted everywhere. Its importance was and continues to be recognized by the agricultural community.³⁵ It gained the biotechnology

³⁴ Committee on Biotechnology, Emerging Biotechnologies in Agriculture: Issues and Policies, Progress Report II, 1983. National Association of State Universities and Land Grant Colleges.

³⁵ J. P. Jordan, Program Administrator, CSRS, "Biotechnology: Promise or Pitfall," pp. 1-10., In F. A.

committee an apparently permanent life within the Land Grant System (it was still in operation in 1995, 10 years after Al Wood's death from cancer). But most importantly, the initial goals that it proclaimed were eventually achieved.³⁶

Soon USDA had its own official biotechnology arm and its competitive grants system was overhauled and expanded to favor biotechnology funding. By the late 1980's numerous public universities had established new biotechnology ventures and centers with the cooperation and funding of their respective state legislatures and with the help of the new competitive grants, even in times of fiscal constraint.

The University of Florida, under Al Wood's guidance, continued its transformation. The idea of a biotechnology building evolved to the concept of dispersing 10 new biotechnology faculty members throughout the departments under IFAS control. Al Wood took a personal interest in the hiring of each of the new faculty biotechnologists, even in late 1984 when the struggle against cancer was fully with him.³⁷ The members of the early Recombinant DNA advisory

Valentine, (ed) Forest and Crop Biotechnology: Progress and Prospects. (New York: Springer-Verlag, 1988).

³⁶ The 1988 Committee on Biotechnology report: Emerging Biotechnologies in Agriculture: Issues and Policies, Progress Report VII, congratulates itself on its successes pp. 1-4, especially in the area of Biosafety regulatory advisement, at the same time as pointing out the need for its continued existence.

³⁷ Personal Interview, the author, with Al Wood,

committee he had formed moved into positions of authority and influence throughout campus.³⁸

Despite his early death, Al Wood lived to see part of the transformation he had struggled so hard to create. New dollars became available through new federal grants. Experiment station dollars more than doubled in biotechnology from 1982 to 1986 and biotechnology faculty at the land grant universities nearly doubled in that same period. The State of Florida and others in the South backed the biotechnology boom.

The much desired increase in industry involvement in the university system flowered as well, nearly doubling from 1982 to 1986. This provided both financial promise and concomitant fears from certain sectors about academic freedom that even today fuel the debate on the role of biotechnology in the future of agriculture. So successful was the transformation of land grant universities that, by 1985, top administrators at USDA/ARS were worried to the point of publicly expressing dismay at the proliferation of biotechnology buildings among the states, decrying, in a sense, their loss of central research control -- all part of

November, 1984.

³⁸ Curt Hannah, one of the earliest members, became the Co-Director of the Interdisciplinary Center for Biotechnology Research (ICBR). It was established in 1987. Most of the others had already or have since achieved prominence in their various scientific disciplines.

the groundswell transformation that Al Wood represented.³⁹

Al Wood's administration had lingering effects. In 1987 the Florida State Legislature provided \$2 million in additional funding for competitive biotechnology research grants. From then on, the University of Florida had both a Biotechnology Institute for Technology Transfer (for cooperation with industry) and an Interdisciplinary Center for Biotechnology Research (which provided interdepartmental cooperation university-wide). Both organizations participated in a key synthetic conference titled "Biotechnology: Science, Education and Commercialization: An International Symposium" held in 1987 and conceptualized by Al Wood. It focused on issues of funding, industry/university relationships and commercialization. Financing of the meeting was provided by IFAS, the University of Texas at Austin, and seven industrial partners, including Upjohn, Monsanto, and Westinghouse. It was an appropriate mix, given the new vision of privatization. The published meeting proceedings were

³⁹ Excerpt of remarks by T. B. Kinney, Jr. Administrator, ARS-BARC Symposium X, "Biotechnology for Solving Agricultural Problems," Beltsville, MD, May 6, 1985: "Second, I suggest that recommendations for new biotechnology centers -- both at the Federal and State levels may not represent the best use of our limited funds. Rather than investing in brick and mortar, I believe in putting the money directly into research." Research, presumably, under ARS direction.

dedicated to Al Wood's memory.⁴⁰

Four years later, in 1991, not quite a decade after the initial survey report in the "Silver Bullet", total biotechnology spending for all state experiment stations had jumped more than fourfold, from \$41 to \$167 million dollars. Florida was again the largest spender (a position it had lost in the years after Wood's death), budgeting almost \$19 million per year for biotechnology, despite its third place rating in total research dollars, behind both California and Texas. Clearly, by almost any criteria, the transformation in agricultural research Al Wood and others had so vigorously promoted in Florida and the nation was, if not complete, then at least well under way.⁴¹

What remains as the most critical question, not just for Al Wood's case but for agriculture as a whole, is the question of belief. In the light of history, what scientific reason did these scientists and administrators possess for their grandiose claims? A bacteria had been manufactured to make a "human" protein and the world went mad with what it

⁴⁰ The proceedings were published as: I. K. Vasil, (ed), Biotechnology: Perspectives, Policies and Issues, (Gainesville: University Presses of Florida, 1988). It contained a dedicatory portrait of Al Wood and discussed his key role in the preface.

⁴¹ Complete statistics are presented on pp. 13-29 of the Committee on Biotechnology, Emerging Biotechnologies in Agriculture: Issues and Policies, Progress Report XI, November, 1992. NASULGC.

saw as the biological version of the bomb at Hiroshima. Finally after years of casual discussion of the possibilities, of genetic transformation, the reality was upon them. But it was more than that, because it was a bomb dropped in biology, not physics: a bomb dropped in humanity's soul, not its backyard. To Al Wood and others like him, it was the final and absolute proof of the unity of life, and the triumph of the engineering point of view: "This technology is as broad in its application as genetics. And genetics is the basic determination of life. . . . It's permeating from a biological and production standpoint and natural resource and management standpoint, everything we do. That's how far reaching it is."⁴²

No further proof was needed for scientists and administrators like Wood and Fulkerson. For them, this one scientific production had validated and confirmed for all time the most deeply held paradigm in modern American biology, emergent from Darwin through the evolutionary synthesis, transformed by the engineering dreams of Jacques Loeb⁴³ and the synthetic visions of Jacques Monod⁴⁴: one

⁴² Al Wood as quoted by T. Hartley in Gene Studies Prove Fertile Ground at UF, p. B-3.

⁴³ For an excellent discussion of the effect of Loeb on American biology, see Philip J. Pauly, Controlling Life: Jacques Loeb and the Engineering Ideal in Biology, (New York: Oxford University Press, 1987). For his mechanistic ideals in his own words, see: J. Loeb, The Mechanistic Concept of Life: Biological Essays, (Chicago: The University

life, one DNA, and one biotechnology. One grand unity, one grand reduction.

But was the transformation of agricultural research a revolution? Did biotechnology in agriculture "emerge" as a surprising new set of ideas and possibilities to confound the modern world? As this dissertation hopes to demonstrate, the problem with the revolutionary assumption is twofold. There was no dramatic conceptual shift in the so-called biotechnology revolution in agriculture (and arguably in biology as a whole). Instead, the dominant paradigm of life as reducible to mechanism, which originated early in this century and had been most significantly articulated years before recombinant DNA became available, was now technologically empowered.

These new biotechnologies did not so much emerge unexpectedly as much as they were developed for a niche which was already prepared. Other recombinant genetic engineering techniques were in early competition. Many scientists and research programs were dedicated towards developing them. Emergence might have been a notion serviceable to the research rhetoric of administrators peddling technological "soap," but it is unfortunate that it

of Chicago Press, 1912).

⁴⁴ In his Chance and Necessity, (New York: Random House, 1972) Jacques Monod states: "The fundamental biological invariant is DNA." p.104).

is a term too much taken at face value in much of even the most critical analyses.⁴⁵

Frederick Buttel, a rural sociologist and critic of agricultural biotechnology, points out that biotechnology's abilities created companies with products for which society had no demand, therefore demand had to be created. An example might be the case of a new variety of toothpaste -- first the public worried about plaque, then tartar "emerged" as a problem. The public had to be educated about tartar, to become convinced in order to create a new demand.⁴⁶ This may have been the case for the early products of biotechnology in the commercial world, but not for the researchers for

⁴⁵ In Feenberg's Critical Theory of Technology p.8., he addresses just such a problematic. Technology appears reified to both its supporters (instrumentalists) and critics (substantive theorists). ". . . if technology is a mere instrumentality, indifferent to values, then its design and structure is not at issue in political debate, only the range and efficiency of its application. . . . if technology is the vehicle for a culture of domination, then we are condemned either to pursue its advance toward dystopia or to regress to a more primitive way of life. In neither case can we change it: in both theories technology is destiny. Reason, in its technological form, is beyond human intervention or repair." The entire concept of "emerging" technologies demonstrates this clearly. The debate between the Al Woods and the Jeremy Rifkins is one of responding to technological destiny that has emerged like some unplanned and unexpected force of nature. Like it or not -- that is the only issue, not its creation, change or cultural context. And, as Feenberg details in his book, both sides (pro and anti-biotechnology in this case) seem to be victims of the same belief system.

⁴⁶ F. H. Buttel, "Are High-Technologies Epoch-Making Technologies? The Case of Biotechnology" Sociological Forum 4:247-261.

whom agricultural biotechnology was always seen as the potential solution to agronomic problems of yield, disease resistance and economic productivity. Much of the misunderstanding of biotechnology as a research enterprise confused its "emergence" with the explosive "emergence" of Genentech and other entrepreneurial companies as a Wall Street and social phenomenon.

Teitelman in Gene Dreams, considers the biotechnology revolution as having failed due to numerous entrepreneurial companies failing to produce products.⁴⁷ Similarly, Frederick Buttel considers agricultural biotechnology to be non-revolutionary because it did not live up to the hype of "any-second miracles," or "second green revolutions."⁴⁸ Such emphasis on products neglects the important transformation biotechnology created in research programs and methodologies --those very areas in which biotechnology was not revolutionary specifically because of its ability to re-invigorate production based research and a reductionist biology.

The development and acceptance of biotechnology in agriculture, however successful, however many different

⁴⁷ R. Teitelman, Gene Dreams.

⁴⁸ F. Buttel, 1993. Ideology and Agricultural Technology in the Late Twentieth Century: Biotechnology as Symbol and Substance. Agriculture and Human Values 10(2):5-15., p.12.

things were left in its wake, could not be a true revolution. In agriculture, at least, it was a sort of counter-revolution. Reductionism and high technology reasserted themselves against the threatening neo-vitalism and holism of the new ecology, animal rights, alternative agriculture and anti-technology movements.

Thus, the continuing arguments for and against the use of genetically engineered organisms in agriculture can be seen not merely as an economic or public health debate: but rather as the cultural angst over two competing world-views. The debate was not between the nature of conflicting scientific theories, but rather the social validity of the concepts inherent in Western science itself, especially ideas of reductionism and progress, and the vision of nature as something to be conquered or subdued.

The mechanistic/reductionist world view was based on the underlying unity of all life. It argued for the "naturalness" of the unnatural, seeing genetic engineering as different only in extent, not in kind to traditional selective breeding. This was set against the neo-vitalistic view that believed in the existence of a higher level species integrity, and "nature" as an authentic and valid participant in the debate.

Furthermore, as Sheldon Krinsky and Roger Wrubel point out, there was a paradigmatic debate between progress as "the degree to which we can control, accommodate to, or

survive, the forces of nature." It was a movement in which "... progress is not found in subduing or dominating nature. This [latter] group," according to Krimsky and Wrubel, "rejects the premise that the goal of humanity is to make nature more rational -- that is, more predictable and controllable."⁴⁹

Scientific participants such as Al Wood and John Fulkerson early on came down strongly on the side of the mechanistic or engineering concept of life. For all the institutional change Al Wood saw and helped to bring about, he and others like him represented something more. These men were fierce exponents of an intellectual trend towards the philosophy of progress. It was a scientific world view that formed the profound basis for their courage to proclaim the "truth" of agricultural biotechnology before the science itself had even begun to bear them out. Many others in their place stood silent.

And yet, their perspective is really not surprising. Agricultural practice in and of itself, especially in the era of modern high-input farming, demythologizes life and treats it as manipulable product. It has long been realized that farm animals and farm plant varieties are, through their domestication (a colloquialism for genetic selection),

⁴⁹ S. Krimsky and R. Wrubel, Agricultural Biotechnology and the Environment (Chicago: University of Illinois Press, 1996) p. 219.

no longer part of the "natural" world in the sense that their survival off the farm as integrated genetic clusters is impossible.

The chemical research that led to the very problems detailed in Silent Spring and that made the biotechnology solution so attractive was itself a product of this earlier engineering mentality. There were many earlier visions of chemical miracles. Hydroponic farms, clonal production, or protein fermentation factories were evidence of a consistent engineering and "unnatural" trend in post-war agricultural research.⁵⁰ It can be argued that it was not merely rhetoric (despite the obvious political benefits to such a position) that led USDA to insist continually that it had been in the biotechnology business from the beginning. At a deeper level, because of this paradigmatic belief, all that the new biotechnologies provided was a technological empowerment of a consistent research goal and a mechanistic world view.

This dissertation will attempt to show that in agricultural practice the move to biotechnology, rather than being a revolution, was an evolutionary process that helped solve a crisis in confidence in the goals and practices of scientific agriculture. It was a technology that allowed the old version of "scientific agriculture" to ascend with new

⁵⁰ Robert Bud discusses these earlier technological components of biotechnology's history on pp. 100-140 in his The Uses of Life: A History of Biotechnology (Cambridge: Cambridge University Press, 1993).

vigor. High-input, high-technology agriculture, the entire engineering-mechanistic view of life and farming, re-emerges against the threats of holistic/organic/pseudo and real ecologically minded "small" agriculture that was threatening it.

Independent of any judgments of scientific validity on one side or the other, battles with perceived Rifkinites⁵¹ may be most easily interpreted as battles between the mechanistic-reductionistic-engineering point of view and the forces of a developing neo-vitalism. This strongly manifested in popular opinion, some aspects of the popular "ecology movement" and much of the "animal rights" campaigns. In this scenario, "real" scientists saw themselves pitted against the pseudo- or unscientific masses. When neo-vitalism merged with anti-technology "fanatics" the theoretical framework for the enemies of science seemed complete. Given the historical moment of its adoption and the fervor of its spread in agriculture, it may be more important to discuss the biotechnology "counter-revolution" than the more common cliché. For whichever

⁵¹ Jeremy Rifkin has been and remains at the forefront of the anti-biotechnology, anti-technology movement in his role as author of books such as Algeny and as head of the Foundation on Economic Trends. He has achieved status as a much-interviewed television celebrity and as a litigant in anti-genetic engineering cases (such as Foundation on Economic Trends v. Heckler, and Foundation on Economic Trends v. Block (see pp. 37-39, Committee on Biotechnology Emerging Biotechnologies in Agriculture Progress Report IV, November 1985)).

paradigm one prefers, neo-vitalism or neo-mechanism, it is not difficult to determine in which one the new vision of genetically engineered farm plants and animals finds the most hospitable home.⁵²

Discussions of the revolutionary nature of biotechnology have been lively and tend in modern analysis more and more towards an evolutionary view than the earliest histories and rhetorical claims of the participants defined. The Spring, 1993 Agriculture and Human Values (Vol. X, no. 2) for example, is devoted to the topic "Biotechnologies and Agriculture: Technical Evolution or Revolution." In that volume, Frederick Buttel in his article "Ideology and Agricultural Technology in the Late Twentieth Century: Biotechnology as Symbol and Substance" argues that: "While the ideology of biotechnology is in some sense a logical extension of the productionist ideology of post-War agricultural science, its rise cannot be accounted for merely by public researchers having embraced biotechnology as the inevitable next phase of productionism." He ultimately comes down on a re-interpretation of biotechnology as developing "in evolutionary (and not revolutionary) fashion from the petrochemical/green

⁵² In his interesting recent book, David Channel The Vital Machine (New York: Oxford University Press, 1991) argues that these two paradigms -- the mechanistic and the vitalistic may be fusing in a new, "scientific" synthesis, which he calls the "vital machine."

revolution, the fourth major agricultural revolution in world history.⁵³" This merely pushes the idea of revolution back a few decades rather than demonstrating that all of modern agricultural research was an evolutionary expansion of a continuing mechanistic world view coupled to incremental technological advances driven by a strict productionist mentality.

This dissertation seeks to point out that the heritage of productionism extends much farther back in the history of American agriculture and has a deeper basis in biotechnology than Buttel appears to believe -- its much touted "emergence" is very much, I would argue, due to its appearance as indeed being the next inevitable phase of productionism.

Buttel, in complete contrast, sees what he calls: "An Emerging Crisis of Productionism" which biotechnology is unable to save the system from due, in part, to its lack of revolutionary productivity. He sees agroindustry as the only remnant supporter of such a productionist mentality and a social cadre of farm groups, consumers, and environmentalists weighed against it. Such a crisis was a source of agricultural biotechnology's birth and rapid adoption, I will argue, far more than a result of its failures and perceived threats. In addition, the crisis in

⁵³ Buttel, 1993. p. 7.

productionism is nowhere near as modern as Buttel portrays, nor as convincing. This position will be discussed in the final chapter, where it will be suggested that biotechnology remains the rhetorical defender of productionism even today. Agroindustry remains the chief partner of the research establishment, not from a modern shot-gun wedding, but due to the mechanistic/production mentality in agriculture to which the research community has adhered from the start.

CHAPTER 2 THE "SCIENTIFICATION" OF AGRICULTURE AND THE BIRTH OF INSTITUTIONAL IDEOLOGY

Although George Washington called for a national agricultural organization in the late 1700s, he was forced, with others who saw the needs of agriculture as paramount to the new republic, to make due with local organizations.¹ Suspicions of federalism ran deep and prevented, along with the early financial crisis faced by the new country, the creation of any centralized authority for agricultural research/promotion. The true force for grass-roots private and state movements for organizing agricultural research in the United States did not arise until the early part of the nineteenth century. It derived from the continuing development of the "scientification" of agriculture and the bringing back home of the new European agricultural chemistry fused with the ideals of the German and Edinburgh experiment stations.

¹ See USDA, A Century of Service: The First 100 Years of the United States Department of Agriculture (Washington D. C.: USDA, 1962), 1-12. The book details the push for a federal role in agriculture during the colonial era. George Washington serves as a starting point for many of these narratives; an obvious utility in a founding myth is to use the "Father of Our Country." Additional validity is found in detailing the role of Thomas Jefferson as an agricultural stalwart and proponent of an agrarian Eden.

Europe provided the first model of scientific agriculture for the United States. Advances in plant and animal husbandry, and most especially the development of agricultural statistics set the stage for government intervention in agricultural research and the development of scientific ideals to transform art into an experimentally derived practice. In the early nineteenth century the development of agricultural chemistry provided the unifying motif for "scientific agriculture." It inspired an ideology of practice that acted as a spur to government intervention. Men like Humphry Davy and Justus Liebig contributed to the production of the new "sciences" and technologies, especially in the development of fertilizers, the earliest pesticides and later plant and animal physiology and pathology.²

Issues of national prestige and world markets coupled to the career interests of individuals in both science and politics conspired to create new institutional entities founded on European science and American political ideology. Agricultural "elites" convinced of the primacy of the

² Numerous studies detail the development of the European experiment station movement and the development of agricultural chemistry. D. Knight, Humphry Davy (Cambridge: Blackwell Publishers, 1992), 47-50, details how Davy's 1813 lectures, published as Elements of Agricultural Chemistry remained the standard until they were superseded by Liebig's Chemistry and its Applications to Agriculture and Physiology (London: L. Playfair, 1843). Chemistry was considered the key to developing pesticides and fertilizers that would help delay Malthusian catastrophe.

democratic process and the educability of the American farmer, created organizations whose dual functions were to convince the average "client" of the value of the new science, at the same time as amassing the resources and political will to produce it.

In the United States, the European ideology, coupled with American nationalism and the image of agrarian utopias, led to the earliest movement for American experimental farms/stations. Since then the goal of "scientific agriculture" has been a critical component of the American agricultural movement.³ The ideology was missionary in the dual sense of practical service and education, but most especially in the zeal with which it assumed that the average farmer would embrace the new doctrines when awareness spread of the true potential of the new approach over the old, non-progressive ways.⁴

³ In their discussion of the "emergence" of agricultural science, H. C. Knoblauch et al. State Agricultural Experiment Stations: A History of Research Policy and Procedure (Washington, D. C.: USDA Miscellaneous Publication 904, 1962), 1-19, point out that it was only with the development of experimentation from Bacon on through the European experiment stations did scientific agriculture come into its own. The American agricultural system absorbed these ideas and promoted them, ultimately leading to the creation of the State Agricultural Experiment Stations (SAES).

⁴ The original ideas of many in the experiment station system were to use farmers themselves to house and perform certain of the experiments. As pointed out by Knoblauch et al., p. 6, "Farmers who regularly conducted trials would, the Directors believed, in time become responsive to new

Numerous reviews of these early days of the development of the agricultural research establishment are available.⁵ A number of them are the result of Centennial celebrations, first of USDA itself in 1962 and then of the State Agricultural Experiment Stations in 1987. Landmark agricultural legislation affecting not only the research establishment but agriculture as a whole has been reviewed by the United States Senate Committee on Agriculture, Nutrition and Forestry from 1825 to 1986. Although these histories fulfill the roles of institutionally self-serving chronicles, they do serve as an excellent probe to the culture and priorities of these establishments, how they interpreted their history, positive and negative, and, by extension, planned to design their futures.

According to N. A. Kerr, one of these chroniclers, the 1840 publication of Leibig's Organic Chemistry in its Application to Agriculture and Physiology had profound, if

ideas, alert to find them, and ready to apply them." Later demonstration farms and mass publications of research results would provide the education necessary to convince the farmers to change their practices to more scientific farming methods.

⁵ See: Knoblauch et al, State Agricultural Experiment Stations (1962); True, A. C. and Clark, V. A. The Agricultural Experiment Station in The United States (Washington: USDA Office of Experiment Stations Bulletin 80, 1900); USDA, A Century of Service (1963), and N. A. Kerr, The Legacy: A Centennial History of the State Agricultural Experiment Stations (Washington, D. C.: Missouri Agricultural Experiment Station, University of Missouri-Columbia and CSRS/USDA, 1987), among others.

often regrettable effects on the American farmer and the agricultural research establishment. It led to an American obsession with "the soil analysis craze" that left the field to charlatans "when the predicted benefits proved illusionary as qualified chemists began to realize the true complexities of soil analysis."⁶ Margaret Rossiter points out that this period translated to the equating of agricultural science with agricultural chemistry -- and would engender continued suspicion on the part of farmers once burned.⁷ As Kerr summarizes: "The renewed interest in soil chemistry may have been short-lived in the general farm population, but it was of lasting importance to America's nascent scientific community."⁸ Would-be scientists travelled to Europe where they were exposed to Liebig's teaching laboratory at Giessen and the new institutions on the continent and Great Britain which combined field or garden testing with laboratory analysis.

Samuel Johnson, an American student of Liebig, returned to America to insist that more science was necessary for agricultural improvement, and to push the idea of

⁶ Kerr, The Legacy, 1987. pp. 2-3.

⁷ M. Rossiter, "Organization of Agricultural Improvement in the United States, 1785-1865. In The Pursuit of Knowledge in the Early American Republic, ed. B. C. Oleson (Baltimore: Johns Hopkins, 1976).

⁸ Kerr, The Legacy, 1987. p.3.

agricultural experiment stations. He was met with a backlash inspired by the failed promise of soil analysis, and was pushed instead to participate in one of the first examples of the regulatory roles that state agriculture would take on in protecting the farmer and consumer. In 1857 he would be appointed to analyze fertilizers offered for sale by private dealers for fraudulent composition.

Institutionally, the Federal Government handled agricultural matters through the Patent Office. It began an agricultural program in the 1820s as a distributor of foreign seed sent by American diplomats to Washington. It expanded its efforts with Congressional funding to include the compilation of agricultural statistics. Not until 1862 did a Department of Agriculture become established, and even then it lacked cabinet-level status.

The vision of a national agriculture and the vast need for effort and funding made the involvement of both state and federal government an obvious requisite, providing the first demand for "big science"⁹ (including both large

⁹ In current historiographical parlance "big science" is a phrase describing what is seen as an essentially post-WWII development of massively funded federal science projects involving, of necessity, the collective efforts of large numbers of scientists. See P. Galison and B. Hevly, (eds), 1992. Big Science: The Growth of Large-Scale Research. Stanford: Stanford University Press. It is a physics-biased model inspired by such developments as the Manhattan Project initially and various atomic accelerators thereafter. The scale, logistics and scope of such "big science" enterprises are considered to make them unique compared to the modes of doing science in the past. Although

numbers of researchers as well as money). This vision demanded the creation of extensive federal programs and expenditures, at the very time when states' rights were surfacing as a fundamental issue in the Civil War and finances were strapped due to a wartime economy. As Kerr points out, the idea that science was intrinsic to the funding ideal was implicit in the early language of the House version of the USDA charter -- a strong call for "botanists, entomologists, and chemists to pursue scientific investigations into the principles underlying agriculture."¹⁰ The language was weakened by the Senate even though an educational role was supported by Congress as a whole. This massive push for education led to the creation of the land-grant university system in the Morrill Land-Grant College Act in 1862 at the same time as USDA was born. In addition, the land scrip for the colleges was also promoted for use in the establishment of sites for "experimental farms."¹¹

Not only did the war make obvious the idea of food production as a national issue (arguably one of the first

it is never mentioned, it could easily be argued according to these considerations that agricultural research was "big science" in funding structure, numbers of researchers, and collective scope after the war as well, but, in this case, the Civil War.

¹⁰ Kerr, The Legacy, 1987. p. 6.

¹¹ Kerr, The Legacy, 1987. p. 8.

defense industries), it also permitted manifestation of an increasing assertiveness by a beleaguered federal government, eager to take advantage of the absence of states'-rights advocates from congressional voting on issues of national concern and federalized power. In sheer political terms, the so-called "Farmer's Legislation" allowed the newly empowered Republican Party to pay back its Midwest supporters.

Both the Homestead Act and the Transcontinental Railroad Act were a hitherto unprecedented extension of federal power, rhetorically driven by the needs of agriculture and its link to national health and prosperity. The latter were a prime indication of the oftentimes complementary needs of the so-called Wall Street money powers and American agriculture, despite the resentment on the one hand and contempt on the other that was quick to develop between farmer and banker once the railroads were in place and shipping prices began to rise.

These factors help to explain the peculiar timing of the institutionalization of agricultural colleges through the Land Grant System in 1862, simultaneous with the raising of Agriculture to Department Status in the Executive Branch. Previously it had languished as an arm of the Patent Office, as (institutionally) little more than a seed service for demonstrating Congressional largesse to its constituency.

This simultaneous creation of a state-wide system

provided an institutional camaraderie among "sibling" institutions at the same time as creating the potential for future tensions with the national Department. Once state and federal government were inexorably linked to agriculture and its promotion, it was inevitable that it became a contested ground for competing visions -- rural vs. city, farmers vs. money powers, republicanism vs. tyranny/monopoly. With the re-introduction of the southern states back into the mix, the state vs. federal issue became ever more coherent. The near instantaneous development of the Association of Land Grant Colleges created what might be considered one of the first national lobbying groups to confound the mix.¹²

By 1872 Congress authorized direct annual appropriations to the land-grant colleges, thereby sealing the federal/state mix. This state/federal connection, so filled with tension, was increased in 1887 with the Hatch Act and the development of a National Agricultural Experiment Station System. It was created at the behest of the American Association of Land Grant Colleges to increase government funding in an era of rising research demands. With this development, the American agricultural

¹² The Association of American Agricultural Colleges and Experiment Stations was officially established in 1887 from the institutions largely responsible for the passage of the Hatch Act (1887). These land-grant college representatives originally met in 1883 to promote the establishment of experiment stations at their institutions. See: True and Clark, Agricultural Experiment Stations, 1900. pp 145-148.

establishment became, in a very real sense, a house divided, with the director of the Office of Experiment Stations having research agendas and organizational goals often in direct conflict with the larger institutional vision of the umbrella United States Department of Agriculture (USDA). It is not an exaggeration to argue that USDA was, for most Americans, their sole contact with federal authority, although it would be increasingly mediated through state hands (via the Land Grant Universities (LGUs) and the State Agricultural Experiment Station (SAES) system).

To rural America, agricultural issues were the federal government. The Populist party rose in its attempts to modify agriculture by controlling government.¹³ Later, with the institutionalization of the already extant agricultural extension service, the local character of agriculture and the increasing reach of federal government were combined. The innate contradiction between the interests of federal and state bureaucracies and bureaucrats remained, however.

This continuing struggle for authority was often fought using the rhetoric of scientific agriculture, debating who

¹³ Lawrence Goodwyn, among others, discusses the efforts of the Populists to return democracy to the federal government by regulating big business interests such as the railroads and the "eastern money powers" and to use federal programs to control monetary policy (in this case promote inflation) and create institutions (subtreasury systems) to benefit and transform an agriculture they deemed to be in crisis. See: L. Goodwyn, The Populist Moment: A Short History of the Agrarian Revolt in America (Oxford: Oxford University Press, 1978).

was in the best position to enforce its production, who was least likely to be relevant or effective to the research needs of the nation as a whole. With nearly one voice, however, the agricultural system maintained the need for ever greater funding and ever greater basic research. By the turn of the century the familiar rhetoric about running out of basic research and living off previous research capital without re-investment had become established themes.

The very nature of the subdisciplines recognized and promoted within agriculture at both the experiment station and federal levels pointed to a common scientific, if not political, agenda at this time among research scientists. At all levels the predominance of agricultural chemistry and the later developments of Mendelian genetics consistently reduced "nature" to a series of problems to be solved in a progressive fashion. Nature was dichotomized into friend or foe in a warfare driven rhetoric which had chemical fixes representing the optimal response to each: fertilizers on the one hand, pesticides/herbicides on the other.

In his paper, "James Curtis Booth's 1852 Essay on Biotechnics and the Biotechnological Vision," George Sanford points out that American scientists such as James Curtis Booth advanced the rhetoric that technology was the ultimate road to benefits in national prosperity.¹⁴ He wrote: "Booth

¹⁴ G. Sanford, "James Curtis Booth's 1852 Essay on Biotechnics and the Biotechnological Vision," pp. 101-124

remained confident that scientific scrutiny would transform farming processes into chemical agriculture." Biotechnics aimed at "directing and controlling the growth of plants (and) . . . animals."¹⁵ Critical to the reductionistic/mechanistic notions that would ultimately flower in twentieth century biotechnology, Booth observed that "plants are modified in appearance and special properties by the use of manures, and the products of animals are influenced by food."¹⁶

Sanford makes clear the case that Booth conflated chemistry with life and was responsible, in part, for the chemical controversy that resulted from charlatans' appropriation of the "biotechnics" vision to fleece the farmer. In response, field experiments achieved primary importance compared to the laboratory vision and the experimental farm took precedence over laboratory analysis - a key factor in the road taken by the developing agricultural experiment stations.

Sanford claims that Booth's biotechnics and modern biotechnology are completely disparate entities -- that the

In: Papers on Agricultural Biotechnology, ed. S. M. Genedel, A. D. Kline, A. A. Paulsen, D. M. Warren, and W. F. Woodman, (Ames: Iowa State University Press, 1988).

¹⁵ G. Sanford, James Curtis Booth's 1852 Essay on Biotechnics and the Biotechnological Vision, 1988, p. 107.

¹⁶ G. Sanford, James Curtis Booth's 1852 Essay on Biotechnics and the Biotechnological Vision, 1988, p. 110.

latter is ungrounded in the former and is conceptually different, both in its perceptions of the nature of science, but also in its goals and potentials for practical success within institutional and disciplinary contexts unthinkable to Booth and his contemporaries. To Booth, biotechnics is disciplinarily transcendent, something Sanford claims is not true of modern biotechnology. It is, however, almost a truism among modern biotechnology practitioners that they have more empathy and understanding of their cohorts in zoology, microbiology and botany who are themselves "biotechnologists" than the organismal practitioners in their own disciplines. (This, in part, is one reason for the development of Biotechnology Centers in universities across the country -- as if biotechnology itself were a new kind of "department."¹⁷) The entire core of biotechnology's power rests in its trans-disciplinary expansiveness. Newsletters on genetic engineering, scientific journals dedicate to

¹⁷ According to Al Wood during an employment interview in 1985 with the author, the idea of a Biotechnology building was dismissed at the University of Florida because it was deemed important to house the various biotechnologists in each discipline within their home departments rather than together. This was thought necessary in order to prevent them from working only with each other rather than providing appropriate service to their specialty fields. Wood stated that the biotechnologists would naturally seek each other out if they were separated, so that the needs of the Departments could take precedence over communal housing. And, in fact, this became the case with the development of the Interdisciplinary Center for Biotechnology Research (ICBR) which developed some years later. The center is not so much a building but rather an association of biotechnologists interacting across campus.

biotechnology, corporate biotechnology products and computer databases, transcend organisms, leaping from bacteria to plant to animal within a context of technique, not specialty.¹⁸ The "Human" Genome Project for example, provides funding for research in the genomes of plants, nematodes, and bacteria as well.¹⁹ This was exactly the kind of universal functionality that Booth appears to have envisioned for his biotechnics.

According to Sanford, Booth made the historically appropriate separation between so-called basic research and technology, seeing them, not as a part of a mutual feedback loop, but rather having independent evolutionary lineages.²⁰ Applied science/technology was not intrinsically dependent on breakthroughs in "basic" science. Sanford traces the idea that basic research leads to applied breakthroughs to Vannevar Bush in the 1940s. He then claims this represents the view of modern biotechnology in direct contrast to what

¹⁸ See for example, the newsletter Genetic Engineering News, and the journals Bio/Technology and Biotechniques for the transdisciplinary nature of the fundamentals, tools and products of biotechnology.

¹⁹ M. S. Lesney and V. B. Smocovitis, "Assessing the Human Genome Project: Effects on World Agriculture," Agriculture and Human Values XI(1):10-18. 1994.

²⁰ In this view technologies would lead to new technologies, not requiring an intervening discovery of so-called basic research principles. G. Sanford, James Curtis Booth's 1852 Essay on Biotechnics and the Biotechnological Vision, 1988. p.119-124.

he sees as Booth's vision of a biotechnics that would lead to continued improvements in its own right.²¹ The distinction seems anachronistic on both counts.

As far as modern biotechnology and science's unique "linear" perspective on scientific development, well before Vannevar Bush, back at least to the 1880s and the early years of the agricultural experiment stations, agricultural scientists would be demanding basic research as the key to applied breakthroughs, and condemning the farmer's efforts as misleading and ineffective. In fact, as part of the original discussions in 1885 on the development of the Hatch Act, it was declared that: "No amount of pure scientific learning can make a successful farmer; no amount of practical experience on the other hand can supply the place of scientific culture in departments which are so thoroughly dependent upon scientific investigation. It is by the harmonious union of these alone by which we can reach the

²¹ Bush, because of his influential book based upon his report to President Truman in 1945 (V. Bush, Science - The Endless Frontier (Washington, D. C.: National Science Foundation, 1960)), has become a modern "father figure" for the institutionalization of federal funding for science in the post-WWII era -- translating a Baconian vision of basic science leading to practical benefits into a military and commercial model viable in the 20th century cold-war milieu. In part it is the primacy of physics and engineering in historical analyses (and in cold-war utility) as compared to the biological sciences that lends an aura of authenticity to this view. He is a prominent figure of authority throughout D. Kevles The Physicists: The History of a Scientific Community in Modern America (New York: Vintage Books, 1977).

perfection of development."²²

And if any firmer stance on the requisites of "basic research first" was needed, William Atwater, who has been regarded as one of the first to respond favorably to the area of biological nitrogen fixation and who served as first Director of the Office of Experiment Stations, made the fundamental case in his introduction to the first Digest of the Annual Reports of the Agricultural Experiment Stations.

As regards the scientific character of their work our stations are doing just what the European stations did in their early experience. They are selecting questions of immediate practical interest and studying them in the most direct ways because they feel that they must, and neither they nor their constituencies have found by actual experience how often this method fails. The difficulty is that the seemingly simplest and most pressing problems reach down to the profoundest depths of abstract law; that the things which appear theoretical are at the bottom the most essential, that the practical interests of the farmer require the theoretical problems to be considered first, for the same reason that the foundation of a house and not the wall is the first to be built.²³

As for modern biotechnology -- its oft-repeated history reads more as a history of technology than a traditional history of science (ie. ideas and theories). Machines, kits and technological finessing, are presented as more

²² Department of Agriculture, Proceedings of a Convention of Delegates from Agricultural Colleges and Experiment Stations, 1885. p. 107.

²³ W. Atwater, Digest of the Annual Reports of the Agricultural Experiment Stations of the United States for 1888, Experiment Station Bulletin No. 2. (Washington D.C: Government Printing Office, 1889)p. 17-18.

responsible for current improvements than any fundamental breakthroughs in basic science (beyond the development of the central dogma of the mechanism and structure of DNA in the first place). Developments in gene-sequencing, fermentation, the Polymerase Chain Reaction, monoclonal antibody techniques and recombinant DNA itself are all technologies that lead to new basic science. In fact, for many it is biotechnology's role to create basic science -- up-ending the Bush paradigm, if such it was. Biotechnology's failures (as with the failures of biotechnics), were blamed on inadequacies of basic research, its successes were seen as due to its own technological impetus. Al Wood's calls for more basic research echoed the pattern of Wilbur Atwater nearly one-hundred years before.

Additionally, biotechnics is seen to fail. The benefits to farming derived from agricultural science are ascribed to the farmers themselves. Sanford claims the farmers demonstrated to agricultural scientists the directions to be taken in their research. He cites, for example, that the determination of which fertilizer components and growing practices were most valuable to study had been empirically determined and not "scientifically derived."

As typical of "empowerment" histories which attempt to provide voices for historical actors formerly deemed marginalized or silenced, for Sanford, farmers become agents of their own destiny, not dependent upon a scientific elite.

"Farmers adopted the implements, management systems and commercial fertilizers to meet their own perceptions of need. Rather than technologies impact upon farmers, farmers "impacted" upon technology and science through their own innovations and selection of technologies from a wide-range of alternatives."²⁴

This was perhaps true of the mid-1800s. It ceased to be the case when agricultural science became fundamentally institutionalized and the land-grant and experiment station systems came into prominence by the turn of the century. Plant and animal breeding, plant pathology and veterinary medicine, agricultural engineering, soil chemistry and analysis, irrigation and feed nutritional studies all were beyond the capabilities of farmers to develop or, as overproduction entered the economic picture, to ignore.

Advances in food processing as in milk centrifugation, pasteurization, homogenization and butterfat analysis forced farmers to adapt. Whole subdisciplines of economic analysis exist examining the need for farmers to adapt quickly to technology despite their wishes, if any of their peers/competitors should do so. The transformation of farming into a corporate capitalist system, driving a vast population from the farm and rural communities and impoverishing many of those who remained was surely not a

²⁴ Sanford, James Curtis Booth's 1852 Essay on Biotechnics and the Biotechnological Vision, 1988. p. 109.

matter of farmers "impacting" technology (unless they suffered from subconscious drives toward self-destruction), but vice versa.²⁵ In all, the history of the impact of non-farm discoveries developed at the agricultural experiment stations and in laboratories belies the input of the farmer in the modern "scientific farming" era.

Booth's own failure was self-ascribed to a lack of basic research -- soil structure and the nature of organic material in soils were too little understood for the technological fixes of Liebig's simple fertilizer formulas to be effective. Booth, echoing biotechnologists of the 1980s observed "that chemistry might prove a great benefit to agriculture, no one doubts; but that it has not yet done so is true."²⁶

Biotechnics and biotechnology are not so disparate as they may appear in the light of "failure" of the former and the apparent "success" of the latter. The reductionist-

²⁵ B. W. Marion, The Organization and Performance of the U.S. Food System NC 117 Committee (Lexington: D. C. Heath and Company, 1986) pp. 14-18, discusses the "technology of production" in agricultural economics. "Technological changes, especially those that allowed substitution of capital for labor . . . had the effect of making traditional-sized family farms too small to employ fully a farm operator family. The resulting low net income of these farms provided a strong incentive for the farmer to adjust. In the 1950s and 1960s, the incentive seemed to get bigger or get out." p.16.

²⁶ G. Sanford, James Curtis Booth's 1852 Essay on Biotechnics and the Biotechnological Vision, 1988, p. 113.

mechanist hallmark is the same. Life is translated into chemistry once more -- the master molecule of DNA is the manipulative crux of current dreams of Eden and agricultural transformation as were the organic chemicals of Booth's day. By 1988, when Sanford was writing, the rhetoric of modern biotechnology had softened, but at its birth it had been as exotic and outrageous as any of the agricultural chemists of Booth's day -- and the commitment to the laboratory as the source of innovation remains.

Robert Bud's discussion of the development of biotechnology from the earlier "zymotechnology" goes into much greater detail in describing the evolutionary continuum between agricultural chemistry, agricultural technology, zymotechnology and ultimately biotechnology.²⁷ The complex relationship between "life" and "chemistry," applied vs. basic, technology vs. science plays out throughout the evolution of biotechnology from its predecessors and cannot be pigeon-holed as easily as Sanford would seem to have it.

The overarching unity seems to be the conviction of "science" as the necessary road to understanding. That understanding was seen as the key to ever-increasing levels of control and ever greater possibilities for practical exploitation.

This self-recognized pedigree would be one of the most

²⁷ Bud, The Uses of Life, 1994.

profound historical validations seen by the agricultural research establishment fighting for its own regulatory autonomy by declaring that in essence they were always biotechnologists.²⁸ Sanford and others would say that this was a purely Whiggish appropriation of historical figures like Booth. I would argue that the answer is more complicated. Booth appears to be part of an authentic tradition and culture, not merely an appropriated caricature of the past.

The Land Grant Mission - The Land Grant Memory

American agriculture, especially in the USDA and the Experiment Station System, continuously solicited, produced, refined and distributed its own memory in official and semi-official ways. These organizations never failed to recognize and celebrate an anniversary. They participated in expositions and the production of an annual, nationally circulated "yearbook" to foster and promote solidarity and support. The yearbook was originated by Congressional design to implement the presentation of statistics and research in a more "user-friendly" fashion than the original Agriculture

²⁸ This rhetoric of biotechnology is discussed in Chapter 8. The introduction to the "Silver Bullet" declares: "Biotechnology . . . is not new to agriculture. Biotechnologies . . . have been the center piece of agricultural production for at least 80 years and in a rudimentary way for the past several thousand years." Committee on Agriculture, NASULGC Emerging Biotechnologies in Agriculture: Issues and Policies Progress Report II, November 1983, p. 1.

Commissioner's report. It would evolve over time to be one of the chief representational instruments of both USDA and the SAES. It would simultaneously be reactive and proactive in its presentation and analysis of demands on the agricultural establishment. After WWII its yearly themes, exemplified in their broad titles would come to mark the path of institutional agriculture across a landscape of social and scientific change.²⁹

The very existence of institutional agricultural research in the United States was the result of intense lobbying that helped to produce certain key legislative acts that set the tone for what was to come both legally and rhetorically. USDA and the SAES relied on the Morrill Act (1862) and the Hatch Act (1887) respectively for their creation.³⁰ But the advanced commitment to basic research

²⁹ Titles include: Science in Farming (1947), Plant Diseases (1953), Animal Diseases (1956), Protecting Our Food (1966), Contours of Change (1970), Cutting Energy Costs (1980), Will There Be Enough Food? (1981), Using Our Natural Resources (1983), and U.S. Agriculture in A Global Economy (1985). Thus, the USDA's yearbooks reflected the societal interests and research dimensions within which farming was constrained over time.

³⁰ Most of the self-created histories of the agricultural system contain either a chronology or the actual texts of the various legislative acts that gave it birth in appendices. See for example USDA, A Century of Service: The First 100 Years of The United States Department of Agriculture, 1962. pp. 520-527 (chronology); Knoblauch et al., State Agricultural Experiment Stations: A History of Research Policy and Procedure, 1962. pp. 217-235 (text of acts); and Kerr, The Legacy: A Centennial History of the State Agricultural Experiment Station, 1887-1987, 1987. pp.

and the land grant system by the federal government was predicated on the passage of the Adams Act (1906) which was, itself, the result of the successful creation of an internal history, political will and a popular program by these same institutions and their friends. The Adams Act was unique in that it specifically supported the importance of basic research as a fundamental requirement of agricultural success. Specified sums were established "to be applied only to paying necessary expenses of conducting original research."³¹ The Act was the result of intense lobbying by the research establishment, but predicated on the conviction of success they were able to convey.

By the 1925 Purnell Act, this amount was increased and applied only to expenses for "experiments directly on the production, manufacture, preparation, use, distribution and marketing of agricultural products." Included under the rubric were "such economic and sociological investigations as have for their purpose the development and improvement of the rural home and rural life. . . ." ³²

205-295 (texts of acts). This use of legislative history provides the agricultural research community with both continuity and historical validation, both useful tools in political lobbying and the construction of institutional identity.

³¹ N. A. Kerr, The Legacy: A Centennial History of the State Agricultural Experiment Stations, 1887-1987, 1987, p. 213-215, gives the complete text of the Adams Act of 1906.

³² The "rural home and rural life" provisions would

In 1887, the Association of American Agricultural Colleges and Experiment Stations was formed from a preexisting organization of agricultural colleges that had met since 1883, (with pre-existing ties to a Teachers of Agriculture group meeting all the way back to 1871). With its inception, research agriculture found not only a lobbying and policy arm at the state level, but an additional organizer of memory and institutional identity.³³

As D. F. Hadwiger points out in his The Politics of

have long-term repercussions, creating a counterforce to the industrialization mentality within USDA by forcing concerns about small farmers and rural populations. It was not until the post-WWII era, however, that the massive inputs on productivity would create sufficiently obvious problems to allow this alternative paradigm its day. As the result of the Purnell Act of 1925 and the 1946 Research and Marketing Act "a relative handful of rural sociologists (sixty-three by 1949) were employed in the state experiment stations). . ." (See Kerr, The Legacy: A Centennial History of the State Agricultural Experiment Stations, 1887-1987, 1987. pp. 136-139 for quote and discussion of sociological research in the agricultural system.) It was not until 1961 that an official Office of Rural Areas Development would be added to USDA. In 1972 the "Rural Development and Small Farm Research and Education Act" was passed. It provided a major increase of monies to fund social scientists within the SAES and to encourage interaction with their extension counterparts (The Legacy provides the complete text of the Purnell Act of 1925 (pp. 215-217), the Research and Marketing Act of 1946 (pp. 220-231), and the Rural Development Act of 1972 (pp. 239-243)).

³³ N. A. Kerr, The Legacy: A Centennial History of the State Agricultural Experiment Stations, 1887-1987, 1987, p. 78, details how this organization evolved and changed names over the years to ultimately become the National Association of State Universities and Land Grant Colleges (NASULGC) in 1965. This would be the group whose Biotechnology Committee, under the Chairmanship of F. A. Wood, would provide some of the strongest lobbying support for the adoption of biotechnology throughout the land grant agricultural system.

Agricultural Research: "The bond among agricultural scientists is rather distinctive in being reinforced by common heritage, common academic subdisciplines, linked institutions, and the dependence upon the same political supporters and funding sources."³⁴ This same bond can be seen among their administrators and many of their funding sources and supporters who consisted of large-scale growers and politicians nursed by the land-grant and extension systems in their youth. Later, as farms ceased to be the primary source of agricultural scientists, the land-grant universities remained the fundamental professionalizing arm for most agricultural researchers and the home of most professional organizations, meetings and the most powerful professional lobbying groups such as the National Association of State Universities and Land Grant Colleges (NASULGC).

The proto-association which would ultimately become the Association of American Agricultural Colleges and Experiment Stations (AAACES) (and later evolve into the NASULGC) had, in part, been responsible for the Hatch Act. Thereafter, its members were eager to promote the interests of the institutions it had given birth to (and which they represented). In union with the Department of Agriculture,

³⁴ D. F. Hadwiger, The Politics of Agricultural Research (Lincoln: The University of Nebraska Press, 1982). p.47.

the Association was instrumental in promoting the Experiment Station and land-grant university system at the World's Columbian Exposition in 1893 and the Paris Exposition of 1900.³⁵

By 1900, in fact, the Association was "officially recognized by the Government as an essential part of the experiment station system and has joined with the United States Department of Agriculture in many enterprises in the interests of the stations."³⁶ Such a relationship would continue as this group later evolved into the National Association of State Universities and Land-Grant Colleges (NASULGC). It was this relationship that provided a political base to lobby for the adoption and funding of biotechnology in the SAES system in the 1980s.

From the very beginning, the Association of American Agricultural Colleges and Experiment Stations was intimately

³⁵ A. C. True and V. A. Clark, The Agricultural Experiment Stations in the United States. The Paris Exposition, 1900 (Washington, D. C.: Government Printing Office, USDA Office of Experiment Stations Bulletin 80, 1900) pp. 32-37. details the establishment of experiment stations and the role of what would become the Association of American Agricultural Colleges in lobbying for and passage of the Hatch Act. The title page of the report acknowledges: "Contributed to by the Association of American Agricultural Colleges and Experiment Stations," thereby demonstrating the continued intercalation of this organization with the activities, policies and history of USDA's Office of Experiment Stations.

³⁶ A. C. True and V. A. Clark, The Agricultural Experiment Stations, 1900. p. 84.

involved in the creation of the state agricultural experiment station system, and also as a principal lobbying and liaison group in Washington for the interests of agriculture at the federal level. The closeness of the relationship was indicated by the official publication of the Associations' meeting proceedings as technical bulletins of the Office of Experiment Stations.

One of the earliest directors (1883-1915) of the Office of Experiment Stations, from 1883-1915 was A. C. True. He worked closely with the AAACES and, in part at its behest, served as a chronicler, first for the research arm, then for the extension system of the SAES.³⁷ Such historical presentations served as public relations, organizational unifiers and political tools. The rhetoric of "science in farming," scientific agriculture, the image of the farmer as client and agricultural research (basic and applied) as a national priority was continuously stressed in such works and modified throughout the twentieth century. Epitomes such as "Men Who Came Before" in the 1947 yearbook provide the framework for agricultural researchers and the public to evaluate and applaud the achievements of the past even as they are seen as bridges and promises of present day research.

³⁷ Kerr, The Legacy: A Centennial History of the State Agricultural Experiment Stations, 1887-1987, 1987. pp. 40-44, 58-59.

The creation of a coherent agricultural extension service as the outreach arm of agricultural science served further to promulgate the mission goals and the political purposes and ideology of organized agriculture to the public at large. Initially expanding from the Hatch Act charter to educate the farmer, by the turn of the century the burgeoning extension system was developing out of financial and administrative control. USDA, conferring with the executive committee of the AAACES to coordinate planning legislation to formalize and finance extension work more fully from the federal level. This resulted in the Smith-Lever Act of 1914 which unified extension work between the two organizations.³⁸ It made extension directly accountable to the state colleges of agriculture. The Secretary of Agriculture created an Office of Extension Work with a supervisory role, but the extension committees set up by the States were given on the ground authority for funding and programs under broad guidelines. This decentralization toward the states and the SAES had obviously been in the best interests of the AAACES. In one sense, it would provide a further wedge between the federal department and its state children. But in another, it was to prove the political salvation of USDA and SAES research and appropriations time and again.

³⁸ USDA, A Century of Service, 1963. pp. 81-83.

The intimate relationship and grass roots connections between the local colleges and agents and their farmers, legislators and influential citizenry could be marshalled collectively when necessary to provide the phenomenal clout that the agricultural lobby was to show over the generations. Even more so, however, future employees of both USDA and the land-grant system rose from the ranks of those influenced by 4-H programs, Future Farmers of America and the myriad of extension related youth projects. At the professional level, nine out of ten Ph.D.s in USDA's research arm as of 1969 had received their degree (and indoctrination) from a land-grant university school.³⁹

The extension service reached into the most rural of areas with the promises of "scientific farming" and for many would be one of the few proofs of the benefits (or even existence) of state and federal authority. As it was extension's job to promote and distribute the discoveries and techniques developed by the research arm of the agricultural establishment, it was inevitable that it would promote the visions and the interests of the establishment itself, until ultimately public education would seem the antidote to every problem faced by agricultural researchers. This faith was grounded in historical myths embodied in the concept of democracy itself. It was inherent in the myth of

³⁹ Hadwiger, The Politics of Agricultural Research, 1982. pp. 59-67.

the yeoman farmer, who was intrinsically wise and needed only the knowledge that science could provide to choose rightly. The faith was made a practical bet by the repeated successes achieved through having institutional control of the educational arm itself. Extension specialists were trained, recruited and professionalized by the agricultural research establishment and acquiesced to essentially all of its diverse methods, beliefs and aspirations.

Surviving two world wars and the depression intact, in fact flourishing after the Second World War, the agricultural ideology and rhetoric of the nineteenth century remained strong and grew through the 1960s, soaring with the successes of wartime production, the export of the Green Revolution and the "triumph" of hybrid corn⁴⁰. Crises came and went, but these were presented by the USDA and SAES communications instruments as simply practical problems demanding scientific solutions. Failures were presented as failures of individual practice, to be modified by even more

⁴⁰ As detailed by Hal Hellman in Feeding the World of the Future (New York: M. Evans and Company, Inc. 1972). pp. 99-101, the Green Revolution began in the 1940s with Norman Borlaug's development of disease free dwarf wheat varieties that responded favorably to high inputs of fertilizer and irrigation. Harvests of the new varieties increased up to ten-fold. It was the dependency on fertilizer that caused problems later on when costs rose as the result of the oil embargoes of the 1970s. Hybridization in major crop species such as corn and wheat became a critical technological development in modern plant breeding. Its ramifications are discussed in Kloppenburg's First the Seed, 1988, pp. 91-129, for corn; and Busch et al.'s Plants, Power and Profit, 1991, pp. 125-133, for wheat.

scientific research and implementation, and not perceived as problems of global ideology.

This profound and continuous maintenance of institutional ideology throughout the profession at both state and federal level stabilized an industrial paradigm of agriculture that was to become the chief object of attack in the "crisis" period of the 1970s and 1980s. This paradigm defined the meanings and limits of "scientific farming" and chose Eden's myth of abundance over its myth of Adam when it perforce chose "progress" as its watchword.⁴¹

The work of the stations, from the start, was tied to productivity, but also to a standardization and mechanization that made the industrialization of agriculture with research support almost a given. In addition, the self-professed Baconian ideals of basic research leading to

⁴¹ The word "progress," and the idea of continued forward movement, as in progressive farming, progressive farmers and scientific progress is a mantra occurring throughout publications by the agricultural establishment from the 1800s through the present. A. C. True and Clark, The Agricultural Experiment Stations in the United States, 1900, p. 32, refer to: "the more advanced leaders in agricultural progress in this country" who demanded the institution of experiment stations. Charles Kastner, "Agricultural Biotechnology and Aphis Regulations: A Pathway to the Future" pp. 80-84, In: USDA, New Crops, New Uses, New Markets: 1992 Yearbook of Agriculture, (Washington D. C.: U. S. Government Printing Office, 1992), p. 84 summarizes the modern equivalent attitude: "This progress bodes well for the continued productivity of American agriculture and holds out the hope of new, more productive, environmentally benign tools to feed the world's growing human population." No post-modern questioning of the idea of progress seems ever to have occurred among production scientists and research administrators.

applied results were paramount.⁴²

Agricultural Science and Disciplinary "Unity"

Modern complaints regarding the lack of unity in biology and science as a whole, and studies to demonstrate this disunity as witnessed in the nineteenth century tend to concentrate on the scientific community as embodied in the private-traditional university and the medical establishment and such professional organizations as the American Association for the Advancement of Science. Toby Appel has demonstrated clearly the lack of disciplinary unity of these "elite" scientific practitioners in this period.⁴³

This disunity makes even more striking the unity of biology and, indeed science, as perceived under the rubric of the agricultural sciences and in organizations such as the experiment stations and their various member societies. Plant pathologists, veterinarians, nutritionists, microbiologists, biochemists, entomologists, agronomists, horticulturists and agricultural engineers all participated in a common enterprise at land grant universities -- based

⁴² Knoblauch et al., State Agricultural Experiment Stations: A History of Research Policy and Procedure, 1962. p. 1.

⁴³ T. Appel, "Organizing Biology: The American Society of Naturalists and its Affiliated Societies, 1883-1923," pp. 87-120, In: Rainger, R., Benson, K. R., and Mainschein, J. (eds), The American Development of Biology, (Philadelphia: University of Pennsylvania Press, 1988).

on common goals of crop production. The chemical paradigm that translated life into nutritional inputs and outputs under the laws of conservation of matter and energy unified research vertically from bacteria to man under the rubric of agricultural productivity.

It was thus that experiment stations were institutional entities where the seemingly most diverse sciences were perforce put into routine cooperation and competition in ways unknown to the more "prestigious" academic institutions where bacteriology and botany and zoology and chemistry could maintain a pristine isolation. Unity of goal and unity of administration forced unity of approach and conglomerate thinking in ways that were unique to the land-grant university system. Later, the insularity of agricultural research would be held against it in numerous critiques in the 1970s, but from the start there was a unity within the disciplines of botanical and zoological sciences in addressing the problems of farming.

Such transdisciplinary and cross-disciplinary interactions were seen as the major strength of such institutions. The farm can be considered the first "ecosystem" into which all of the biological disciplines were pulled for a unified approach to common problems via common investigations. It was intrinsic to the list of specialties included in the array of projects sponsored by the 54 agricultural experiment stations extant in 1900.

These included: "horticulture," "diseases of plants," "diseases of animals," "chemistry," "dairying," and "entomology."⁴⁴

Dairy production, for example, required a unification of animal physiology, agricultural chemistry, plant and animal nutrition, bacteriology, economic entomology, soil fertility, human and veterinary medicine and ultimately breeding and genetics. It was considered one of the triumphs of the USDA/SAES approach. A Dairy Division was established in the Bureau of Animal Industries in 1895, as part of the same appropriation which released \$10,000 from Congress for nutrition studies at the suggestion of USDA Secretary Morton.⁴⁵

Plant and animal diseases and their intimate relationship to entomology represented one of the favorite scientific coups that agricultural research liked to claim to its own credit. Equally the conquest of disease represented a key area of biological unification. In its centennial celebration edition, USDA called the "conquest of cattle fever" the "outstanding scientific accomplishment of the period." This proclaimed triumph resulted from the discovery that cattle fever was carried by ticks and proved

⁴⁴ A. C. True and V. A. Clark, The Agricultural Research Stations in the United States, 1900. p. 43.

⁴⁵ USDA, A Century of Service, 1962. p.36.

to be "one that has been of inestimable value to human life." USDA scientists also showed that several plant diseases were insect transmitted. Ultimately this pattern would prove applicable to humans as well, providing a model used by researchers to control yellow fever by controlling the insect.⁴⁶

Such studies linked bacteriology, agricultural productivity, human and veterinary medicine, entomology and field experimentation. It was no accident that such unity would later prove a ready branch onto which the new biotechnologies could be grafted. As importantly, these examples provided a rhetoric of success which would infuse the institutional memory of the agricultural research system at both state and federal level, simultaneously inspiring young scientists and convincing legislative bodies of the value of scientific farming.

Farm problems also provided a means of demonstrating the validity and requirement for agricultural extension education service. The outbreak of cotton boll weevil almost destroyed cotton production in the South, yet the weevil was amenable to well-understood control methods.⁴⁷ The boll

⁴⁶ USDA, A Century of Service, 1962. p. 32.

⁴⁷ The boll weevil was controllable through cultural methods such as fall plowing and burning of stalks after harvest. "In response to the boll weevil, southern farmers welcomed federal agents and the advice of USDA for the first time." See: R. D. Hurt, American Agriculture: A Brief

weevil incident provided a forum for extension education and a demonstrable success that helped fuel the momentum for specialized extension legislation. This culminated in the Smith-Lever Act, which in itself would provide a further forum for agricultural successes through educational propaganda.⁴⁸

The broad mandate of agriculture in dealing with farm and foodstuffs alike provided a tremendous impetus for diversified research interests under the experiment station umbrella. It created additional pillars of community support for agricultural research. Especially highlighted in the 1900 Paris Exposition commemoration volume developed by A. C. True, the Director of the Agricultural Experiment Stations at the time, was the role of nutrition investigations, both human and animal. Here the unity of biology as an outgrowth of chemical reductionism applied to life processes becomes most obvious, as did its industrial applications. As A. C. True reports: "The science of the nutrition of man has so much in common with that of nutrition of animals that a distinction between the two is

History, 1994. p. 226.

⁴⁸ The section titled "Thanks to the Boll Weevil" in J. F. Cooper, Dimensions in History: Recounting Florida Extension Service Progress, 1909-1976 (Gainesville: Rose Printing Co., Inc., 1976), p. 1-2, points out how the boll weevil allowed for demonstration farms in the south which helped forward the extension movement leading to the passage of the Smith-Lever Act in 1914.

not easily made, and naturally they have been often studied together."⁴⁹

Alfred True details the history of such studies from 1803 on in the United States and makes much of the classic in vivo coupled to in vitro studies of Dr. William Beaumont in the 1820s. He traces further developments directly to ash analysis of foodstuffs in the next period, from 1844 to 1860. These are the same analyses that led to the fertilizer agenda of the early agricultural reductionism (e. g. the "biotechnics" of Booth) where chemistry would solve all of agriculture's problems. This work was deemed, by True, only "of interest to-day chiefly from a historical standpoint,"⁵⁰ not, it must be pointed out, because the general idea of chemical reductionism was wrong, but because the appropriate technology had not been discovered and the level of analysis was too crude. According to True, it was only with the discovery of the "Weende" methods around 1864 that "accurate and reliable" organic and analytical methods were available. Using such developments, it was deemed "possible to carry on systematic investigations of food materials from the standpoint of their nutritive values as determined by their

⁴⁹ A. C. True and V. A. Clark, The Agricultural Experiment Stations in the United States, 1900, p. 106.

⁵⁰ True and Clark, The Agricultural Experiment Stations in the United States, 1900, p.107.

chemical composition."⁵¹ In USDA, the Division of Chemistry was responsible for such work.

Amazingly enough, USDA was at the forefront of human experimentation in this period in conjunction with the Department of Labor. If any commitment toward early industrialization and productivity through the use of scientific investigation needed to be demonstrated, this research provides one of the most striking links of USDA directly to the Taylorization movements of the era and to urban concerns well in advance of the perceived abandonment of the "family farm" and a rural clientele.⁵²

⁵¹ True and Clark, The Agricultural Experiment Stations, 1900, p. 107.

⁵² L. S. Reich, The Making of American Industrial Research: Science and Business at GE and Bell, 1876-1926 (New York: Cambridge University Press, 1985) p. 36, details how "Frederick Taylor's principles of "Scientific Management" first developed in the late 1890s, were designed to increase the productivity of both capital and labor by reorganizing production processes among more efficient lines. Management secured the cooperation of laborers by rewarding those who accommodated themselves to Taylor's highly regimented system. Invoking the general principle of "Scientific Management," Progressive reformers expected to make both industry and government run more efficiently." The tie to human health research and intervention is made explicit in P. Starr, The Social Transformation of American Medicine: The Rise of a Sovereign Profession and the Making of a Vast Industry (New York: Basic Books, Inc., 1985) p. 200: "The rise of industrial hygiene and medical engineering were part of the same current that produced the theories of scientific management of Frederick Taylor. . . . Employers had a practical interest in using medical services for recruiting and selecting workers, maintaining their capacity and motivation to work, keeping down liability and insurance costs and gaining good will from their employees and the public."

As detailed by True and Clark, in 1886, Professor Atwater, chief proponent and first director of the Office of Experiment Stations, analyzed the results collected by Carroll D. Wright, chief of the Massachusetts Bureau of Statistics of Labor of typical diets from a number of cities to "enable the working man to regulate more intelligently his expenditures for food and to secure, with a given expenditure, the maximum amount of nutritive ingredients."⁵³ Unsatisfied with the quality of the original data, Atwater later repeated the survey in Middletown, Connecticut and performed actual analysis of many of the foodstuffs consumed, not relying on "typical" analyses as the original study had done.

By 1890 the Connecticut Storrs Station was closely connected to Wright (then U.S. Commissioner of Labor) on a continuing series of such studies. According to True: "By January 1895, twenty-one such studies of the actual food consumptions of families of mechanics and professional men had been made and reported by the station." Nearly 300 such studies were being conducting by 1900 "under the auspices of the United States Department of Agriculture."⁵⁴

Unsatisfied with mere nutritional data obtained from

⁵³ True and Clark, The Agricultural Experiment Stations, 1900. pp. 114-116.

⁵⁴ True and Clark, The Agricultural Experiment Stations, 1900. p. 113.

analysis of the food materials themselves, research quickly passed to the analysis of food digestibility both from before and after consumption studies, concentrating heavily on urine and fecal analysis and calorimetry studies.⁵⁵

Atwater, returned to research from his post as Director of the Experiment Stations in 1891, and worked with Professor Hempel of Germany to improve the use of the Bertholet bomb calorimeter. By studying the amount of energy (calories) given off by burning the original food compared to that liberated by fecal and urine samples, estimates of nutritional efficiency could be made.

To further refine the studies, Atwater developed a respiration calorimeter in conjunction with E. B. Rosa, a physics professor from Wesleyan University, producing a device "accurate enough to justify its use in experiments with men. . . ." ⁵⁶ by which he meant male college students.

It is not giving too great credence to the critics of agricultural reductionism to assert that one of the unfortunate consequences of the biological and chemical unity of life paradigm behind the USDA/SAES research mentality did indeed lead to the conflating of human

⁵⁵ Calorimeters are devices for determining the amount of energy liberated by combustion -- digestion being the metabolic equivalent of a slow burn.

⁵⁶ True and Clark, The Agricultural Experiment Stations, 1900. p. 112.

industrial workers with farm animals -- creatures to be manipulated by proper feeding regimens to highest productivity. The work was used to define rations for the Army and Navy of the United States. Ironically, in a connection that would delight Michel Foucault as historian, Atwater would also become nutritional consultant to the Commission of Lunacy of New York in order to use the new science to improve the diets of the insane patients in State hospitals.⁵⁷ The humanitarian results of improved diets first for workers then for "consumers" were subsequently considered key accomplishments of USDA nutritional programs and vigorously self-praised in various yearbooks.

These nutritional studies were well in keeping with the sanitation and progressive movements of the time. Revisionist American historians have argued these reforms were not primarily altruistic movements formed in response to abuses, but rather experiments in progress and

⁵⁷ In his Madness and Civilization: A History of Insanity in the Age of Reason (New York: Random House, 1965) and The Birth of the Clinic: An Archaeology of Medical Perception (New York: Pantheon, 1973) Michel Foucault uses both madness and disease respectively to demonstrate how society developed government and scientific practice (institutions, doctors and physiologists in these cases) to exert power over individuals by sequestering (often by imprisonment) and regulating the treatment of perceived disorders in the human mind or the human body respectively. Foucault does not see altruism at the core of these movements but rather the development of social order and control. If seen as an ultimate outgrowth of Taylorization, Atwater's attempts to bring nutritional health to the mentally ill can be interpreted as an ironic fusion these Foucaultian visions.

efficiency.⁵⁸ In this view, these "scientific" social improvements formed the rational basis for the transformation of individual capitalism to the corporate capitalism of the modern era. If this is indeed the case, the USDA/SAES was intimately involved in such enterprises from their start.

But the importance of their scientific discoveries as "basic research" cannot be dismissed. In the applied details of such work, men like Atwater and True were convinced that they were finding the reductionistic core of biology. As True pointed out, later experiments furnished "definite,

⁵⁸ Daniel T. Rogers "In Search of Progressivism" In: Kutler, S. and Katz, S. (eds) Reviews in American History: Progress and Prospects, Vol. 10, No. 4, 1982, p. 145, details the complexity of the progressive movement, but points out that the "Left" re-analyzed Progressivism in the 1960s and discovered in it the seeds of corporate capitalism. The real story, Rogers contends, was: "how, in the face of a major crisis of legitimacy, the leaders of corporate capitalism had managed to capture dissent itself. What others called 'progressivism' and 'corporate liberalism' was at bottom . . . a series of measures designed to draw the teeth of the rank-and-file militancy and middle-class resentment - an abandonment of the cutthroat ethics of entrepreneurial capitalism for new ideals of social harmony and new schemes of business-government cooperation which would insure a social order safe for the new corporate phase of capitalism. Insofar as progressives dreamed of social efficiency . . . they attested to a brilliantly successful capture of ideas and policies by the most powerful organizations around. . . ."

Ronald Radosch, "The Myth of the New Deal" In: Radosch, R. and Rothbard, M. (eds) A New History of Leviathan: Essays on the Rise of the American Corporate State (New York: E. P. Dutton, Inc., 1972) argues a similar case for the use of reforms and government regulation to streamline business efficiency and protect the corporate state from popular unrest.

accurate information concerning the action of the fundamental laws of the conservation of matter and energy in living systems. . . . These results are exceedingly valuable from the standpoint of both pure science and practical utility."⁵⁹

These were some of the earliest aspects of "big science" of which the agricultural research establishment was so proud, and which it would continually tout in self-commemorations. Agricultural research administrators were proud of the multiplicity of studies, disciplines and sites capable of being brought to bear upon a single problem to create applied results from basic research. It seemed a particular delight for True to point out at the international Paris Exhibition that the "Atwater-Rosa respiration calorimeter" was being expanded large enough to use for domestic animals and was catching on in both Bonn and Budapest. America was beginning to pay back its debt to Europe for the original development of the experiment station system in the eighteenth and early nineteenth centuries.

An important component of this multi-disciplinary mentality fostered by research administrators was the credit system in which scientists could alternately share. The spotlight could continuously move, guaranteeing each

⁵⁹ True and Clark, The Agricultural Experiment Stations, 1900. p. 113.

discipline its representative "honor" or primacy depending on its percentage role in solving any particular problem. Each could point to its unique successes, and each could point to unique collaborations with other disciplines to both validate their own role and that of their neighbor. A growing budget and an administrative commitment to "fairness" (but also to protecting all the members of their team) provided agricultural science with a unique power base in both research and political maneuvering that was to prove immensely successful -- so successful that, in the crisis years post-WWII, its very claims to success would be held against it. The universal application of the solutions developed by the experiment station system and the land-grant university system would reveal the unexpected flaws in their fundamental design.

If they had not been so successful (in initial results for one thing, in political empowerment for the agricultural research community for another) their methods would have never been so extensively applied as to have caused the problems that would lead to crisis. The early systems approach that so lent itself to industrial application was at the heart of that success. The research increasingly constrained the small producer and put USDA and the experiment station system in the roles of quality assurance. Optimization of inputs meant not only standardization of inputs (granting regulatory power) but also the requisite

facilities to use those inputs to compete. As with all innovations, both the initial costs, the starting conditions, and economies of scale dictated which farmers or consortia could most profit, and encouraged the growth of the more profitable enterprises and the diminution of the less. In this case, knowledge was power, at least power in the marketplace.

Despite the USDA and the SAES commitment to communication with the small farmer and with the public as a whole, it was inevitable that the larger concerns and the industrializing farms systems would be most in a position to take immediate, and hence permanent advantage of the research done. In fact, in a peculiar construction of its own history in this period the USDA centennial "celebration" edition depicted the 1890s as the watershed for the development of the industrial paradigm of mass production. The historical analysis was, however, twisted by this logic of production. Secretary Morton was originally brought in, to determine whether the USDA/SAES research and regulatory activities were "in the interest of the farmer and the public" and to drastically reduce them, even to the extent of eliminating the Department, if he deemed them unworthy. But, like his predecessor, he was led toward "one goal - expansion of production research." For, ". . . what he discovered was that events both inside and outside farming were emphasizing the demand for more, rather than less,

research and regulation."⁶⁰

In the USDA version of its own history, the Panic of 1893 and the depression in farm prices were given as reasons that made it "imperative for farmers to cut costs and increase efficiency if they were to stay in business." The threat of the Populists to older parties was mentioned. "The answer, or at least one answer, to many of these problems was to make the farmer a more efficient producer.

Efficient production would cut unit costs and thus, theoretically at least, increase profits. At the same time more good food would become available at reasonable prices to city dwellers. Farmers would become more efficient as the result of research by the Department, State colleges, and State experiment stations. The Department was to take the lead in the most effective emphasis upon farm production research the world had ever seen.⁶¹

No statement could better set out what its opponents would call the blindness of the paradigm of abundance, the paradigm of improved production through research that motivated USDA and the agricultural establishment from its inception on. According to many historians, the farm depression after the panic is blamed as much on "overproduction" as on any other factors, and on the tremendous amounts of food produced through the planting of the Homestead Act lands and its availability to city markets

⁶⁰ USDA, A Century of Service, 1962. p. 37.

⁶¹ USDA, A Century of Service, 1962. p. 37.

by railroads criss-crossing agricultural America.⁶²

Much Populist dissent was an attempt to encourage production curbs by banking excess production until a more economic time in order to drive up prices, not provide "reasonable prices to city dwellers." In the developing economic war between rural and urban interests, the research arm most dedicated to rural interests had already chosen sides in its accent on production: industrialization was the key method toward efficiency, and "reasonable prices to city dwellers" would be as great if not a greater guide to research policy as any benefit toward the producers themselves.

They [Farmers] found no consolation in the realization that these growing urban centers provided

⁶² J. O'Sullivan and E. E. Keuchel American Economic History: From Abundance to Constraint (New York: John Watts, 1981) p. 103, states: "In the South cotton production expanded beyond the needs of the market and, by the 1890s, could not be raised profitably on many of the sharecropping and tenant operations. Overexpansion in the West resulted in a price collapse aggravated by drought and other natural disasters. . . ." In discussing the validity of complaints against the railroads, Fred A. Shannon The Farmers Last Frontier: Agriculture, 1860-1897 (New York: Harper and Row, Publishers, 1945) pp. 295-296, states: "When the farmers . . . complained that it took the value of one bushel of corn to pay the freight on another bushel, or when the farmers . . . said the same of wheat, often this was no exaggeration and sometimes it was an understatement." Standard textbook accounts of modern American history also blame both overproduction caused by expansion of lands and new technologies, as well as the price of railroad shipping (which also put farmers in competition with each other and with foreign markets) for the farmer's problems in this period. See: B. Tindall and D. Shi, America: A Narrative History, Fourth Edition (Volume Two) (New York, W. W. Norton, 1996), pp. 947-963.

markets for the increased farm production. Instead they saw themselves becoming a minority in an America where they had once been dominant. The demoralization of the rural communities and the small towns that served them proved one of the most destructive aspects of the generalized adjustments being made to the new commercialized agriculture. Successful farmers tended to be the larger, more efficient operators, while the greater number of small farmers found it increasingly difficult to turn a profit.⁶³

It was a scientific utilitarianism that aligned more easily with the conservative American Farm Bureau than with the Populists, with the cornucopia model of Eden over the agrarian joys of the rural Adam. As the SAES celebratory edition for the USDA centennial proclaimed: "... food abundance is a reality in the United States. This development, stimulated by science and education, is now recognized as perhaps the greatest step forward in the history of civilization."⁶⁴ The paradigm of productivity through science, having lead to this self-proclaimed "greatest step forward" in history, was unlikely to be abandoned.

Agricultural science worked ultimately toward the Taylorization of science. In such mode it ignored organizational niceties of disciplinary boundaries, common in the private universities. It borrowed eclectically from

⁶³ J. O'Sullivan, J. and E. E. Keuchel, American Economic History, 1981. p. 104.

⁶⁴ Knoblauch et al., State Agricultural Experiment Stations, 1962. p. 1.

all sciences, treated human and animal biology alike, and reduced to physics and chemistry the supposed complexities of life in order to take an engineering approach to solving the "problems" of "scientific farming" and efficient production. Critics of the agricultural research establishment would consider this mentality the single largest cause of the production crisis that would gut the original small farmer clientele. The embrace of this mechanistic/reductionist mentality would create the openings for attack by alternative paradigms, some of the most powerful of which, were nursed within the bosom of its own success.⁶⁵

⁶⁵ Even the opening of new lands for production in the West (considered one of the other main causes of overproduction) was due, in great part, to earlier technological developments in farming. The McCormick reaper (close to 80,000 sold by 1860) allowed the mechanized harvest of vast tracks of land by fewer laborers. According to J. O'Sullivan and E. E. Keuchel, American Economic History, 1981. p. 75: "The productivity increases generated by the reaper reduced demand for farm labor and encouraged the eventual shift of large numbers of workers from agriculture to industry." The U. S. Government perceived this production as an unalloyed good and attributed such an attitude to farmers in general (though surely the displaced farmers and farm workers might have disagreed). O'Sullivan, J. and Keuchel, 1981. p. 75, indicate that "The census of 1860 . . . noted McCormick's contribution when pointing to 'the evidence of improvement in some of the most important agricultural operations, proving that our farmers are fully in sympathy with the progressive spirit of the age, and not behind their fellow-citizens engaged in other industrial occupations.'"

CHAPTER 3
SILENT SPRING, VOCAL CRITICS:
THE AGRICULTURAL RESEARCH SYSTEM UNDER ATTACK

The 1970s were a time of transition in US agriculture. Failures of the old science and the old technology led to the demand for new answers, a new paradigm from external critics and agricultural researchers alike.

Almost all scholars and reviewers¹ of the subject cite several key influences on the agricultural system at this time. First, in 1973 the Hightower Report: Hard Tomatoes/Hard Times lambasted California agricultural scientists for betraying the small-farmer and migrant laborer to agribusiness. Research scientists in plant breeding in concert with agricultural engineers had helped to develop a late-ripening tomato that would allow for the use of large-scale mechanical harvesters. This development created a crisis for migrant laborers who were no longer needed. Smaller farming operations, including many of the sacrosanct "family farms," could not afford the new machinery and were forced out of production by agribusiness, which could. The consumer, furthermore, although

¹ See for example: F. Buttel, "Biotechnology and Agricultural Research Policy: Emergent Issues," pp. 312-347. In: Dahlberg, K. A. (ed) New Directions for Agriculture and Agricultural Research (New York: Rowman & Allanheld, 1986).

theoretically treated to higher productivity and lower prices, was faced with a tomato, that despite chemical ripening techniques, was considered both tasteless and hard. This was in comparison to earlier varieties in which taste, not mechanical properties, had been a primary issue of selection.

In these and countless other instances, the land-grant university system seemed less a national public resource but more a "tool" of corporate capitalism, helping promote the interests of agricultural industrialization to the detriment of their supposedly "traditional" clientele.

Simultaneously the fruits of "Silent Spring" were ripening and the traditional pesticides that had formed the basis of the post-World War II technological boom were defeated (as with DDT) or were under severe threat.² A tremendous public outcry blamed agriculture for much of the pollution woes, creating, for almost the first time in the existence of the land-grant system, a wide-spread public hostility toward much of "modern" agricultural research.³

The core concept of agricultural chemistry, which might be considered the mechanistic "soul" of the modern agricultural paradigm, was most severely under attack with the development of a new holistic ecological mentality, both

² R. Carson, Silent Spring (New York: Fawcett, 1962).

³ L. J. Lear, "Bombshell in Beltsville: USDA and the Challenge of Silent Spring, Agricultural History 66: 151-171 (1992).

"scientific" and "popular." The impact of Rachel Carson served to solidify already existing fears that the increased use of pesticides and fertilizers had been raising in the minds of the agricultural researchers themselves. The pesticides that had formed the basis for the post-World War II technological boom, such as DDT, were under severe threat and some were ultimately abandoned without adequate substitute. As pointed out by L. J. Lear, in Bombshell in Beltsville, whole research programs and careers based in the developments of new pesticides and herbicides seemed jeopardized. Researchers as much as farmers were in a panic over what would happen when too many of their favored chemicals were outlawed. The realization that substantial changes in research direction were necessary forced both an institutional and scientific crisis on organized American agricultural research.⁴

As the Pentagon would react to anti-Vietnam War protesters, by attempting to discredit them at every turn as kooks and traitors, so would USDA react to repudiators of the dreams of chemical control in the "warfare" against plant pests, despite its own scientists's private fears. Representatives for both industry and academia made exaggerated claims of safety of some of the most toxic chemicals, and raised nightmare scenarios of Malthusian doom

⁴ L. J. Lear, "Bombshell in Beltsville: USDA and the Challenge of Silent Spring, Agricultural History 66: 151-171 (1992).

and starvation should they be abandoned.⁵

Because of the demand for public safety and pollution control, there were entirely new regulatory schedules and a new agency, the Environmental Protection Agency (EPA), to deal with. This was all part of a strong push for change in the way agriculture was done, especially influenced by Carson and the new ecology movements. As a new agency, the EPA proved neither reticent nor politic in its demands to intrude upon agricultural research. Early demands to "audit" laboratory notebooks of land-grant university scientists to confirm pesticide safety studies as being accurate and scientific created outrage among faculty, administrators, and industrial funding partners alike. Such demands were an unprecedented breach of scientific trust between institutions and cries of "fascism" and "communist authoritarianism" were equally hurled at the new "enemy."

In November, 1977, I. Gard, Manager of Field Development of the AGCHEM Division of the Pennwalt Corp (based in Fresno, CA) sent a mailgram to various directors

⁵ F. Graham, Since Silent Spring (Boston: Houghton Mifflin Company, 1970), p. 48-68, details the "Counterattack" against Rachel Carson's book. The National Agricultural Chemicals Association doubled its public relations budget and claimed Carson's book was "a serious threat to the continued supply of wholesome, nutritious food . . . as a result of recent unfounded, sensational publicity with respect to agricultural chemicals" (p. 58). Various "fact kits" on the value and safety of properly used pesticides were mass mailed to medical practitioners by chemical companies as part of a public relations rally against the book.

of State Experiment Stations. Vernon Perry of the Office of the Dean for Research (IFAS) passed it on to all unit heads for faculty comment to be forwarded to EPA:

A PROPOSAL REQUIRING AUDIT OF ALL EXPERIMENT STATION DATA USED FOR REGISTRATION PURPOSES IS CURRENTLY UNDER CONSIDERATION BY EPA. THIS PROPOSAL REQUIRES RETENTION OF ALL RAW DATA NOTES, AND RECORDS FOR THE LIFE OF THE PRODUCT: TESTS NOT HAVING THESE DATA WILL BE CONSIDERED INVALID. WE FEEL THIS UNREASONABLE APPROACH MAY INVALIDATE MUCH GOOD BASIC WORK DONE IN THE PAST AS WELL AS FUTURE DATA. WE SUGGEST WORKERS AT YOUR STATION(S) FILE COMMENTS. . . WITH EPA. . . .⁶

And comments there were, most overwhelmingly unfavorable, for example -- J. P. Jones: "I would never willingly permit any representative of the EPA or any other. . . regulatory agency to audit my experimental data. . . . I am sure you are aware of the Lysenko situation in Russia."⁷ and W. G. Genung: "I resent the H... out of EPA or any other NKVD type agency peeking over my shoulder. . . . I can imagine such distrust and challenge to individual honor coming out of the Soviet Union, Red China, or Nazi Germany. But, here! In the good old USA. . . ? If that thinking is an end product then why did we spend billions. . . to crush Nazi Germany? Why Korea? Why Vietnam?"⁸ It is telling to

⁶ Mailgram from I. Gard to V. Perry, November 4, 1977. Gainesville: University of Florida Archives.

⁷ J. P. Jones, November 17, 1977. Letter to EPA. Subject: Pesticide registration inspections. Gainesville: University of Florida Archives.

⁸ W. G. Genung, November 21, 1977. Letter to D. L. Myhre. Subject: EPA pesticide registration inspections.

note that the EPA was seen as a foreign enemy--either fascist or communist. Equally, the unity between industrial concerns and academic ones was notable. Pennwalt requested the responses and IFAS rapidly complied.

But it was not only ecological concerns and the inspiration of "Silent Spring" that led to massive and vocal criticism of agricultural research. Plant breeding itself, one of the seemingly inherently unobjectionable technologies, had revealed critical flaws that no one had expected. For example, by the late 1970s, the Green Revolution in developing nations had begun to founder due to its inherent dependence on Western-style, high-input agricultural practices. Thus a major success in traditional plant genetics was parlayed into an apparent disaster. Dwarf, high-yielding grain varieties had been developed in an environment of cheap fertilizers and were dependent on them, not only for their high yields, but to outcompete traditional varieties, which often performed as good or better on less than optimal soil and water conditions. Norman Borlaug had received his Nobel Peace Prize in 1970 for his role as foster-father of the new high-yielding grains -- an award given while hope was still high for agricultural miracles. It was thus considered a triumph of agricultural research both politically and scientifically, so the claimed "failure" of and attacks on the Green

Revolution were considered knifeblades at the heart of Western-style productivity research per se.⁹

But such failure had practical ramifications on world politics and not just on agricultural prestige. Population explosion rhetoric at the time was a staple of both political policy and the popular press, exacerbating the fears and increasing the blame that the failure of the "new miracle grains" evoked.¹⁰ The so-called "Green Revolution" failed, ostensibly, due to rising prices during OPEC oil embargoes.¹¹ Because most fertilizers were made using oil-

⁹ For a discussion of the development of the Green Revolution see J. H. Perkins, "The Rockefeller Foundation and the Green Revolution, 1941-1956," Agriculture and Human Values Vol. VII (No. 3/4): 6-18 (1990). Perkins discusses the development and early history of the Green Revolution both scientifically and politically. As was typical for production research in the United States, research focused solely on increasing yields and ignored implications of distribution, input costs, social effects or sustainability.

¹⁰ In G. O. Barney, The Global 2000 Report to the President (New York: Penguin Books, 1982), p. 16, the population growth and the high costs of green revolution productivity were considered a dangerous mix: "The projections indicate that most of the increase in food production will come from more intensive use of yield-enhancing, energy-intensive inputs and technologies such as fertilizer, pesticides, and irrigation - in many cases with diminishing returns." Malthusian population growth was feared. Although temporarily forestalled by the increased production, disaster seemed inevitable in the 21st century.

¹¹ Some claim that too much has been made of the supposed failure of the green revolution, asserting that because of it, countries such as India were able to become, at least temporarily, grain self-sufficient, and even capable of export. This is considered part of the problem by researchers such as Vandana Shiva in Monocultures of the Mind, (London: Zed Books, Ltd., 1993) who portrayed this as a more profound form of neo-colonialism, destroying crop

based technology or extensive energy inputs, poor farmers could not afford to follow through on the intensive management practices (especially fertilizer) that the new varieties required. Thus, along with the use of pesticides, fertilization, the second half of "better living through chemistry" was under threat.¹²

According to existing data, at the turn of the century, one farmer in the United States could feed seven people. By 1982 the ratio was one farmer feeding more than 78 people. Total agricultural output in the U.S. had more doubled since the 1930s, though the agricultural land base remained

diversity and creating greater dependence on international markets dominated by the industrialized West (pp. 39-49). Lester Brown in By Bread Alone, (New York: Praeger Press, 1974, pp. 133-145) considered the green revolution of temporary benefit but severely constrained by the energy crises and international food marketing and distribution.

¹² Fertilization by this time was also recognized as a pollution problem -- increasing nutrients caused nitrification of lakes and rivers, increasing algal bloom and damaging local ecologies. In J. W. Sites, Research Program Planning Memorandum: 1975-1978 (Gainesville: Institute of Food and Agricultural Sciences, 1975) p. 118-120, the need for controlling fertilizer runoff to prevent water pollution and means to achieve control is discussed. Such issues however seemed to have had less dramatic impact on the public imagination which was fired by the anti-pesticide rhetoric of dead robins and endangered bald eagles (F. Graham, Since Silent Spring, 1970, pp. 81-92). The perceived failure of and the spread of the green revolution, however, had geo-political effects and raised images once more of mass starvation. The International Rice Research Institute's Dr. M. S. Swaminathan (quoted by Jack Doyle Altered Harvest: Agriculture, Genetics and the Fate of the World Food Supply (New York: Viking Penguin Inc., 1985), p. 278, stated: "the net cereal deficit of 36 million tons in 1978-1979 will have doubled by 1990 and again doubled by 2000."

roughly the same.¹³ This phenomenal result was almost solely due to scientific input into the age-old methods of growing food. Heavy, energy-intensive, chemical-assisted, vigorous land use and highly mechanized techniques were developed in the post-war decades. These high-input methods, coupled to biological research and development of the plants themselves were the source of the oft-touted "miracle" of modern agriculture.

But on a global scale, and nationally, a massive destruction of resources was becoming obvious due to those very high-technology processes which sacrificed bushels of topsoil to wind and erosion for every bushel of corn produced, that poisoned land and water (with irrigation, fertilizers and pesticides) and promoted the encroachment of desert everywhere through overuse of soils and water, through replacement of traditional crops with the more intensive "Western" varieties.¹⁴

¹³ J. A. Zellner and R. F. Lamm, "Agriculture's Vital Role for Us All," pp. 2-9, In: USDA 1982 Yearbook of Agriculture (Washington, D. C.: United States Department of Agriculture, 1982).

¹⁴ R. N. Sampson, Farmland or Wasteland: A Time to Choose -- Overcoming the Threat to America's Farm and Food Future (Emmaus: Rodale Press, 1981) discusses in great detail the soil, water and social costs of high-input agriculture in the United States and touts the need for "sustainable agriculture." It should be noted that Rodale Press acts as a powerful outlet for critics of industrialized agriculture. Rodale also publishes the magazine, Organic Gardening, which strongly supports the farm ecology movement. V. Shiva, V. Monocultures of the Mind: Perspectives on Biodiversity and Biotechnology (London: Zed Books Ltd., 1993) p. 9-59, discusses the

More input became required for less gain as fuel, fertilizer, and equipment became scarce or too expensive in the developing world. The economics rapidly became such that high-input technologies, even while they remained workable in the developed nations, were untenable for the developing nations for whom they were originally designed in Food for Peace¹⁵ programs and Borlaug's "Green Revolution." Current population levels were projected by government and private agencies alike to be rapidly rising without hope of non-traumatic curbs. Agricultural chemistry's promise of miracles from fertilization, an ideological pillar of "scientific agriculture," now seemed economically untenable. Fears of Malthusian nightmares re-emerged despite falling US farm prices from American overproduction.¹⁶

cultural, social and environmental impacts of the new technologies in farming on third world agriculture.

¹⁵ Post World War II United States food aid was packaged under the idea of "Food for Peace." Food aid was considered a political as well as humanitarian tool for stabilizing nations at threat to communism due to economic chaos, hunger and poverty. As described by R. G. Stanley in Food for Peace: Hope and Reality of U. S. Food Aid (New York: Gordon and Breach, 1973) p. x.: "Food can be an important avenue to accelerate economic development; it can also be a weapon to insure peace." A critical component of this aid was not the delivery of food surplus alone, but the attempt to provide the technological wherewithal for developing nations to feed and clothe themselves. Thus the export of technologies such as the Green Revolution was necessary to the goals of this program and its successors.

¹⁶ According to Barney, Global 2000 (1982), p. 18-19, real costs of producing food increased roughly 10% in 1978 and 1979. This was coupled to major overproduction problems for the farmers and by the embargo of grain sales to the Soviets imposed by President Carter as a rebuke for their

Closer to home, in 1970, another triumph of plant breeding had blown up in agricultural research's collective faces. Southern Corn Leaf Blight struck the United States and vast acres of corn were levelled by this fungus-caused disease. It alone accounted for the destruction of 15% to 20% of the entire crop in this country and parts of Canada.

The unique feature of this particular disease outbreak was that it served as a grim reminder of the naturally occurring risks that modern day agriculture seemed forced to accept in its search for ever increasing yields. The epidemic occurred as a direct result of a massive, finely coordinated genetic modification scheme carried out on American corn to decrease production costs and improve yields. The disease occurred because of a nearly complete dependence ^{on} the benefits of the genetic uniformity possible through the use of hybrid corn. ✓

Corn, like many other plants, showed what was known as "hybrid vigor." This meant that the best seed a farmer could plant was hybrid seed (the product of the union of male and female sex cells from two different genetic varieties, specially chosen by breeders to complement one another to greatest effect). Plants from such hybrid seed showed

invasion of Afghanistan. Lesser Developed Countries (LDCs) were unable to buy the overproduction at prices acceptable to US farming costs and interests, thereby creating the simultaneous problems of too much food in the developed world/not enough in the underdeveloped countries. It would remain a permanent problem warping idealized supply and demand markets.

greater vigor in terms of growing characteristic and were much superior generally to those from seed produced by the union of male and female sex cells from the same plant or same variety.

There is an extensive literature on the development of hybrid corn as one of the first seed-based "biotechnologies" of the modern era, including much discussion in books such as First the Seed of whether it was indeed biological necessity, or rather corporate profit, that led to the development of hybrid corn.¹⁷ It is part of the scandal to which agricultural research was also subjected when there was evidence that a prime reason for the shared development of such hybrids with industry was to force farmers to buy the hybrid seed from companies rather than save their own seed, as was the historical practice when corn was just "varieties."

The largest cost in the production of hybrid seed was the need for extensive use of manual labor to detassel (remove the male portions of) the "mother" variety. This had to be done before the plant could fertilize itself or other nearby "mother" plants (as would normally happen in a farmer's field where food production and not special seed

¹⁷ The issue of hybrid seed is discussed extensively in J. Kloppenburg, First the Seed, 1988. pp. 91-128; J. Doyle, Altered Harvest, 1985. pp. 32-45; V. Shiva, Monocultures of the Mind, 1993. pp.27-58; and Busch et al., Plants, Power and Profit, 1991. pp. 62-66; and D. Fitzgerald, 1993. "Farmer's Deskilled: Hybrid Corn and Farmer's Work, Technology and Culture 34(2):324-343. among many others.

stock was the goal). Once detasselling was completed, only pollen from the chosen "father" variety would have a chance to fertilize the "mother" plants which would then reproduce the superior, hybrid seed that farmers bought for planting.

As agricultural engineers would remove the migrant laborer through development of machine harvesting techniques in tomatoes, so the plant breeders eliminated migrant labor in the detasselling process. Through careful selective breeding, they developed lines of "mother" corn that contained the CMS (Cytoplasmic Male Sterile), Texas or "T" Type cytoplasmic gene.¹⁸ This gene conferred the characteristic of pollen sterility: thus corn plants that had this gene could only act as females.

A "father" variety could be planted in close proximity to these all-female plants and its pollen would have no unwanted competition. Cheap and plentiful hybrid seed was guaranteed without the involvement of time-consuming and costly hand labor for detasselling.

However, problems arose with a vengeance. A new disease appeared. It turned out that a weakly parasitic fungus,

¹⁸ For an description and review of cytoplasmic male sterility, including type T see: C. S. Leavings, "Cytoplasmic Male Sterility" pp. 81-92, In: Kosuge et. al. (eds) Genetic Engineering of Plants: An Agricultural Perspective (New York: Plenum Press, 1983). CMS was of extreme interest to genetic engineering because it was an economically important single gene-directed trait and relied on scientifically interesting complex interactions between the genomes of the mitochondria and the nucleus of a single cell which found expression only in flowering tissues.

never before a significant problem in corn, suddenly became a predator of prime importance. It proved capable of producing a plant-killing toxin that affected only corn carrying the CMS(Type T) gene. And an incredible amount of corn had been planted with the CMS-T gene.¹⁹

The planting of a single crop (such as CMS-T corn) of a genetically uniform variety is called monoculture. It was another of the good/bad techniques for which modern agricultural practice would ultimately be castigated, both on the grounds of genetic diversity, but also for fear of the catastrophes that Southern Corn Leaf Blight had proven both possible and unpredictable.²⁰

Jack Kloppenberg relates how, in the standard fashion of research administrators, after the problem was "fixed" by returning to older cytoplasms, E.T. York, founder of the Institute of Food and Agricultural Sciences (IFAS) at the University of Florida, tried to claim credit for the land-grant system for its quick response (a response to a problem it was highly instrumental in creating).²¹ This last bit of disingenuousness in helping to solve a problem that they had

¹⁹ Kloppenburg, First the Seed, 1988. p. 122. points out that fully 15% of the 1970 corn crop was destroyed due to the disease attacking this single variety planted in extensive monoculture.

²⁰ Kloppenberg, J. First the Seed, 1988. p. 122. suggests evidence that researchers in plant breeding, both public and private, had hints of a possible problem in 1969 and kept their mouths shut.

²¹ Kloppenburg, First the Seed, 1988. p. 122.

provoked in the first place did not seem to take completely and the entire incident was a "blight" on agricultural research as much as it was on the corn. Although it would be used consistently as an example of the dangers of monoculture, the experience with Southern Corn Leaf Blight did little to stop the direction of research, overall, however -- genetic uniformity having such economic potential. The blight did, however, provide a boost to a new disciplinary subset in plant pathology involved in the study of plant toxins.²²

Genetic uniformity of height, harvest date, nutritional and water requirements, and even the disease resistance possible (at least initially) provided one of the clearest advantages that breeders were capable of contributing to the new, high-input farm. Neatly ranked arrays of near-clones allowed every section of a field to be treated in the same way, the first necessity of mechanization. And mechanization of irrigation, fertilizer and pesticide application, and finally harvest was what led to maximal yields with minimal labor costs. It was another case where the industrialization

²² See: R. P. Scheffer, "Host-Specific Toxins in Relation to Pathogenesis and Disease Resistance," pp. 247-269. In: Heitefuss, R. and Williams, P. H. (eds) Encyclopedia of Plant Physiology (New York: Springer-Verlag, 1976). Later the behavior of plant toxins would prove a useful model for plant genetic engineering and the molecular biology of plant disease in later years. See: O. C. Yoder, "Use of Pathogen-Produced Toxins in Genetic Engineering of Plants and Pathogens" pp. 333-354. In: Kosuge, T. et al. (eds) Genetic Engineering of Plants: An Agricultural Perspective (New York: Plenum Press, 1983).

of agriculture was pursued despite the risks--and another cause to question the priorities of the agricultural research establishment in this period.

Monoculture methods were developed for plantation production of coffee, bananas, cocoa and tea. They resulted in thousand acre farms of corn, wheat and other cereals, vast fields of lettuce, potatoes, and sugar beets, huge orchards on the massive scale that industrial agriculture required for maximal and cost-efficient production.

Southern Corn Leaf Blight would be a stark (though rarely mentioned) warning to the future genetic engineers who would look to plant cloning and the incorporation of single gene resistance into crop species, risking just such catastrophes of genetic monoculture once again. Despite these dangers, it was the great economic potential of monoculture outlined above that made so many of the techniques of genetic engineering, plant cloning and cell culture to be discussed in the next chapter so valuable. If nothing else, the disease provided a ready market for the services of plant pathologists in the new biotechnology world.

In the 1970s, gas and oil shortages in the U.S. threatened not only agricultural productivity but research productivity as well when universities were forced into conservation mode and an unprecedented shift to low impact/low energy agriculture was mandated across the board.

At the University of Florida, as reported in an article in the Gainesville Sun, the University of Florida developed an Energy Crisis Center, "the first of its kind in the United States," in order "to pool information about the energy crisis and channel it to industry and Government."²³ This followed efforts by UF, typical of other institutions at the time, to limit its own energy usage. The state as a whole worked to limit agricultural use of gas and oil, working with the Florida Fruit and Vegetable Association (FFVA). By Feb. 1974, UF Vice-President for Agricultural Affairs, Ken Tefertiller (later to be important in the development of biotechnology at UF and the NASULGC), announced the extension education program in energy conservation for Florida agriculture. According to an IFAS press release dated February 6, 1974, it "represents the first step in the program whose theme is 'Agricultural Growth in an Urban Age.'" This was being funded by a \$150,000 grant from the USDA Extension Service.²⁴

Additionally, in the United States that same revolution in plant breeding produced grain surpluses and threatened the family farm with a drastic loss of revenue that exacerbated the effects of increased production costs caused by an inflationary spiral.

²³ Gainesville Sun, Gainesville, December 2, 1973.

²⁴ IFAS press release (2/6/74). Gainesville: University of Florida Archives.

Not only was practice under attack on the farm front, but in the ivory towers of agricultural enterprise as well. The land-grant colleges and the SAES had suffered severe blows and a fall in public confidence. They were blamed for these "failed" technologies that produced overproduction at a harsh environmental price, and were held to ridicule in successive government reports by the Office of Technology Assessment and other prestigious bodies such as the National Science Foundation.²⁵ The entire agricultural research and education system was cited as a haven for applied agricultural hacks, any ability to do or train in basic research compromised.

What cannot be underestimated is the self-perceived role of agricultural researchers as "scientists" in a larger scientific community. Agricultural scientists felt tremendous peer-pressure resultant from the need to seek prestige and career validation from their scientific peers. This was especially true of researchers in a university setting. In the institutional dynamics of large bureaucracies, subpopulations can have disproportionate effects on the entire community due both to unique internal

²⁵ A descriptive bibliography of such attacks is provided by Schweikhardt, D. B. and Bonnen, J. T. "Policy Conflicts in Agricultural Research," pp. 13-27. In: Busch, L. & Lacy, W. B. (eds) The Agricultural Scientific Enterprise: A System in Transition (Boulder: Westview Press, 1986), and in Buttel, F. H. "Biotechnology and Agricultural Research Policy: Emergent Issues," pp. 312-347, In: New Directions for Agriculture and Agricultural Research (New York: Rowman & Allanheld, 1986).

dynamics and to peculiarized sensitivities to outside pressures. Basic research scientists and research administrators, both at the state and federal level, were taking the external criticisms to heart that they were "research hacks" who could only do applied research because, for this community, external power relations and external validation had become far more important issues than the satisfaction of a diminishing client group.²⁶

Perhaps as importantly from an agricultural crisis perspective, the very possibilities of the old scientific approaches were seen in danger of becoming unproductive. Numerous researchers pointed toward an increasingly asymptotic approach to apparently "fixed" productivity levels in major crop species, leading to fears of ultimately diminishing returns in any increasing application of current technologies. Silvan Wittwer, of Michigan State University, one of the most respected agricultural science policy mavens, pointed this out as a demand for more fundamental

²⁶ Hadwiger, The Politics of Agricultural Research, 1982. pp. 51-67, points out that scientists often work for reasons of professional demands of peers rather than external ties to clientele groups. Much of the "crisis" can be put down to these attacks from scientific peers. However, it cannot be ignored that these peer evaluations were not mere psychological accolades or condemnations. Funding decisions by congress were becoming increasingly tied to such expert evaluations and the traditional formula funding of agriculture was in danger of being diminished by such attacks and demands for institution of "competitive" research funding.

research.²⁷ In addition, he was critical of the USDA/SAES system for having avoidable administrative constraints to performing such research due to excessive staffing, redundancy and over-regulation.

Thus the world of agriculture, on the farm and in the land grant university, was suffering a crisis of confidence both scientific and political, private and public. The handwriting was on the wall with such issues as "Growth in an Urban Age," used to title a report which attempted to adapt agricultural research, extension and teaching models to a new urban and industrial clientele whose chief interests were cheap food and fiber, new products, and no pollution.²⁸

The old funding and support systems were diminishing, the previous rhetoric directed toward the old clientele was failing to appeal to an increasingly industrial, large-scale and urban population. In this light, betrayals of the family farmer lambasted in the Hightower Report in 1973 can instead be seen as the first recognition by the system that the traditional clientele was being not so much left behind, as recognized to be increasingly irrelevant, both as forces of production, but also as potential sources of political

²⁷ S. Wittwer, "Agricultural Research" pp. 315-317, In: Busch, L. & Lacy, W. B. (eds) The Agricultural Scientific Enterprise: A System in Transition (Boulder: Westview Press, 1986).

²⁸ Institute of Food and Agricultural Sciences, Agricultural Growth in an Urban Age, 1975.

clout.

Market forces and inflationary pressures seemed to demand massive productivity gains. The Malthusian fears that spurred the Global 2000 report commissioned by President Carter demanded a new science and a new industrialization.²⁹ Food became a political weapon and international markets and global trade a fundamental concern as it had never been before.

If all this were not enough, the omnibus Farm Bills begun in 1973 altered the direction of, and internal make-up of the USDA every four years in line with the changing demands of Congressional politics rather than scientific necessity (or even consistency). In 1972, with the Rural Development Act, Congress mandated the largest increase in research and extension in rural development since the Depression. This provided a contradictory force against service geared to an ever industrialized/urban clientele

²⁹ G. O. Barney, The Global 200 Report to the President: Entering the Twenty-First Century (New York: Penguin Books, 1982), p. 16, points out that increased food production in the future will not meet demand and rather than relying on new lands brought into cultivation "most of the increase in food production will come from more extensive use of yield-enhancing, energy-intensive inputs and technologies such as fertilizer, pesticides, herbicides, and irrigation - in many cases with diminishing returns." Notably these necessary technologies included those which created the most notable problems in American agriculture -- pollution, high prices, increased mechanization and costs, as well as diminishing returns. If something did not change, the standard American agricultural science would increase yields, but not sufficiently to stave off crises of both hunger and pollution on a global scale.

towards which the system was rapidly evolving.

In 1977 Congress passed The National Agricultural Research, Teaching and Extension Act as Title XIV of the Food and Agricultural Act.³⁰ It forced USDA and the land-grant institutions to come to terms with the earlier criticisms by establishing new joint planning boards to examine and guide long term policy in research, teaching and extension. It mandated the establishment of competitive grants "oriented toward basic research and available to researchers outside the traditional land-grant institutions."³¹ At the same time it created non-competitive grants for small farms, energy and low-income nutrition programs.

This shift in government, especially toward the end of the decade, only crystallized a problem that was increasingly obvious throughout the 1970s to basic researchers and administrators alike: the old systems were rapidly evolving and the basic research was not keeping up with the political and economic changes. This did not even begin to consider the new worlds of science being opened in biology as a whole.

Financially, what was clear from NASULGC meetings is

³⁰ D. L. Stansbury, "The Context and Implications of the National Agricultural Research, Extension and Teaching Act of 1977," pp. 132-157. In: Dahlberg, K. A., (ed) New Directions for Agriculture and Agricultural Research (New York: Rowman & Allanheld, 1986).

³¹ Kerr, The Legacy, 1987. p. 243-295.

that the unexpected Reagan Revolution by 1981 had constricted the money supply for non-defense related research and for education in general. This followed a period of near-depression in the smaller farm communities. Equally critical to the administrative structure of the agricultural community were strident calls from "respected" outsiders such as the Winrock Foundation for agency streamlining, elimination of bureaucratic and research redundancy and a nationalized prioritizing of basic research -- demands nearly all inimical to the existing goals and structures of the USDA-SAES system and the personnel and funding needs of individual researchers.³²

By 1985 Frederick Buttel and others were describing the effects of these crises and criticisms on agricultural researchers and the land-grant community. He was one of the first to recognize that biotechnology would lead to "the apparent passing of what were a short time ago called the 'new agenda' items (e.g., the impacts of public agricultural research on farm structure, environmental quality, and rural communities). " In fact, these were the very same issues which led to the developing power of his own sociological subdiscipline in institutionalized agriculture.³³ He held

³² F. Buttel, Biotechnology and Agricultural Research Policy: Emergent Issues, 1986, p. 314-318.

³³ F. Buttel, "The Land-Grant System: A Sociological Perspective on Value Conflicts and Ethical Issues." Agriculture and Human Values Vol. 2, March 1985. p. 36.

faith that these issues would reappear, although "redefined and reshaped by the very emergence and development of biotechnology." He recognized, that for the moment, however, the institutional impetus had moved from the problems to the solutions.

These problems did demand solutions, as far as the research community was concerned. They were part of research planning and the daily thinking of administrators and scientists alike. For example, in 1979 in an address to the Florida State Horticultural Society, Al Wood enumerated the problems as they appeared to an LGU research administrator:

For a variety of reasons, we are expected to maintain and even increase our horticultural production and to do this with fewer chemicals for the control of pests and use as fertilizer, with less water, on less productive soils than we had access to in the past and in general with less energy. To make matters worse, this set of constraints exists during a period in which we are experiencing a staggering inflation rate.³⁴

These same constraints were seen to impact the research program -- but all the while with a concomitant demand to do "science."

So critical to the changes that were effecting traditional agricultural research in the LGU community was this new emphasis on solving problems using basic research and peer-accredited science. The production of refereed publications as an obvious method of monitoring scientific production among agricultural researchers soon became "one

³⁴ Wood, Address to the Florida State Horticultural Society, November 8, 1978. p. 2.

of the most important criteria used as a basis for promotion and tenure decisions."³⁵ Grower satisfaction through non-refereed research articles and university teaching diminished in importance as critiques of the LGU as a "hack" research enterprise hit home.

Evidence of the strain that this created in the previous system was presented in correspondence between Wood and university scientists and external client groups as to the definition of a "refereed" publication, and why "important" publications such as the Proceedings of the Florida State Horticultural Society should be considered "equivalent" to "refereed." This effort to convince failed for the simple reason that it was not this clientele group that was at interest here, but the respect of academic peers and those scientists who had leveled deadly criticism against the quality of USDA and land-grant research.

One such letter to Wood with a copy sent to the Secretary of the Florida State Horticultural Society was from David Mall on January 5, 1979. Mall was a Consultant for Cornucopia Products. He complained that Proceedings papers passed a "reality test" in terms of usefulness that other publications such as Phytopathology did not. Mall cites the standard canon: "I am definitely not against "pure" research, that is the basis for new Knowledge, but we

³⁵ Wood, Address to the Florida State Horticultural Society, November 8, 1978. p. 7.

must have enough application of this research to pay the bills today. The purpose of building the "ivory tower" (pure research) is so we can see to the far horizon (applications), not just so the tower's occupants can talk to those in the next tower."³⁶ One could hardly find a clearer recognition of the differing needs of client groups and scientific professionalization--an issue of divided loyalties which was tearing at the heart of the agricultural research system in this period.

Wood used the majority of his address to the Horticultural Society explaining why publication in the Proceedings, though important, was no longer good enough. It was a deliberate distancing from former client interests to larger research priorities and academic prestige. The old loyalties, if loyalties they truly were, crumbled rapidly under the new pressures, consistent problems and attacks that the agricultural research system now faced.

In all, therefore, due not only from the compounded effects of "Silent Spring" but from the ever increasing number of vocal critics from all sides, both in the scientific community and in the general public alike, it was not a good time for what many had called the "most successful agricultural system in the world." Something had to be done. From one perspective this period can be seen as

³⁶ Letter from David Mall to Al Wood, dated January 5, 1979, (Gainesville: University of Florida Archives).

a typical "Kuhnian crisis" where failures of the old science and the old technologies led to the demand for new answers, a new paradigm. Abandoning old clientele and shifting to high-tech research, especially biotechnology, would be seen to provide that answer for agricultural scientists and especially for research administrators such as Wood who sought to return the land grant system to its perceived glory days.

CHAPTER 4 THE NEW BIOLOGY: A DNA WORLD

The "Revolution" in Molecular Biology

Agricultural science predominately evolved as a mechanistic, reductionistic form of biological science--a paradigm necessary for its utility as a series of essential principles for engineering. A large component was always a form of "molecular" biology in the sense of chemical approaches and genetic manipulations. It bore the strongest allegiance to a Baconian model of science, where basic research leads to applications of practical importance. Though there has been some argument about the motives of the early Mendelians in U.S. agriculture--that they were interested in the science qua science, in botany and genetics not agriculture--this was never the attitude of their research administrators and funders, and certainly not the public view of agricultural science at large.¹ Some of

¹ From the beginning, the desire to do basic research competed with the demands of farmers and state officials for practical demonstration research and for regulatory analysis of soils, fertilizers and pesticides, food quality, etc. Those administrators who pushed research over teaching, and basic research over applied, still had the attitude that this was the first requisite step to improving agriculture in the long run, not to assuage scientific curiosity or to develop professional advances for their own sake. D. B. Paul and B. A. Kimmelman "Mendel in America: Theory and Practice, 1900-1919," pp. 281-310 In: Rainger et al., The American

the most significant discoveries in basic genetic theory were made by individuals whose ultimate practical intent was never in question. Time and again, as shown in previous chapters, basic research was seen as a critical necessity, but ultimately it was justified because it produced applied breakthroughs. Increasingly, the basic research pointed toward the chemical nature of life, especially in its ultimately practical results.

But it was not until after WWII, after the evolutionary synthesis and the concretization of the central dogma of biology, that complete molecular reductionism was possible as an ideal--and a fundable ideal at that.² This chapter will deal with those aspects of biological science in agriculture that, before recombinant DNA techniques became generally applicable, created the institutional and

Development of Biology (London: Rutgers University Press, 1988) detail the gamut of interests in Mendelism from the most applied breeders to the theoretical geneticists. As they point out, agriculturalists were "scientists of a particular kind, both institutionally and in respect to their aims. Employed at agricultural colleges and agricultural experiment stations. . . they were concerned with the implications of Mendelism for practice as well as theory." (p. 283).

² As Warren Weaver, director of the Rockefeller Foundation's Natural Science Division stated in talking about the new DNA technology: "The century of biology upon which we are embarked is no matter of trivialities.... This is the dependable way to seek a solution of the cancer and polio problems.... This is the knowledge on which we must base our solution of the population and the food problems. This is the understanding of life." (Quoted by Ronald Rainger in his Introduction to The Expansion of American Biology, p.1.).

ideological milieu in which such technologies would thrive and be embraced. It will concentrate primarily on the plant sciences, the most neglected disciplines in historical examinations of developments in biotechnology, but more importantly the source of the most powerful and promising of the new technologies for agriculture as whole.

V. B. Smocovitis argues cogently that the gross unification of biology was accomplished through the evolutionary synthesis and that by the late 1950s a consensus of disciplines and institutions took place.³ This unification was incomplete, however, in some respects, primarily because methodological or "interest" problems remained. Some biological disciplines felt excluded, in that, while granting the overarching role of evolution in all biology, they found it still too remote to be applicable to their daily pursuits, including methodological or experimental approaches. There was no obvious link between evolutionary time and the laboratory observations of physiological and molecular processes. Two things were required to bridge this gap: the development of the Central Dogma and the creation of a valid field functioning in the role of Richard Goldschmidt's "physiological genetics" as a bridge between those disciplines studying phenotypes v.

³ V. B. Smocovitis, Unifying Biology: The Evolutionary Synthesis and Evolutionary Biology (New Jersey, Princeton University Press, 1996).

those studying genotypes.⁴ This would be one of the reasons for demanding more basic research in plant science as a prerequisite for the use of biotechnology.

In 1953, the structure of DNA was reported by James Watson and Francis Crick, and the possibilities for faithful replication inherent in that structure, coupled to its foundation as the long elusive genetic material, gave new impetus to a molecular reductionism that had been thriving in laboratory settings since the birth of organic chemistry and the discovery and understanding of enzymes. Moving rapidly from previous vitalism to ever more mechanistic models of biochemical action, molecular biology provided a promise of biological understanding and practical application that was nearly irresistible to a new generation of biologists seeking to remove from their science the stigma of being natural history or "mere" systematics to a model more like that of physics, or most especially, the chemistry which seemed now to be the real source of any

⁴ The "central dogma" as it is called, was first developed by Francis Crick (See: H. F. Judson, The Eighth Day of Creation (New York: Simon and Schuster, 1979) p. 335-339). It detailed the necessity of information passing from nucleic acid to protein and not vice versa. "Physiological genetics" or "Physiological Theory of Heredity" was the science proposed by Richard Goldschmidt that would explicate the expression of genes through metabolism leading to an individual phenotype (See: E. W. Sinnott and L. C. Dunn, Principles of Genetics: A Textbook, with Problems (New York: McGraw-Hill Book Company, Inc., 1932) pp. 333-334.

"elan vital."⁵

This attitude was especially true for many agricultural scientists who were increasingly recognizing their foundation in agricultural chemistry. They were also the inheritors of an early Mendelism which was strikingly pervasive in USDA and the Land Grant Colleges from the very first years after the "rediscovery" in 1900.⁶

The central dogma provided a foundation for the genetics that had grounded the evolutionary synthesis, but equally important, it replaced the bag of enzymes model of the cell -- a particularly intransigent study object with scarcely plausible connections to any useful hereditary model -- with the primacy of the DNA molecule as the universal explicator of genetic phenomena. Not only did this development complete the unification of biology, but it led from this unification to a profound reductionist conviction of the unity of science on the basis of a kind of molecular determinism. This attitude imbued the developing molecular biology community. The inexorable link between physics and

⁵ Garland E. Allen, Life Science in the Twentieth Century (New York: Cambridge University Press, 1978), p. 425-427, points out the reductionism inherent in the discoverers' interpretations of DNA, especially Francis Crick, whom he quotes as writing in 1966: "The ultimate aim of the modern movement in biology is in fact to explain all biology in terms of physics and chemistry. . . . Thus eventually one may hope to have the whole biology 'explained' in terms of the level below it, and so on right down to the atomic level." (p. 425).

⁶ See: Paul, D. B., and Kimmelman, B. A., Mendel in America, 1991.

chemistry had long been assumed. Now with biology promisingly reducible to the biochemistry of the gene (i.e., DNA) the validity of biology as a hard science in the hierarchy of the sciences and its ability to enter into the realm of biological engineering (biological technology becoming biotechnology) was assured.

Agricultural researchers on a practical level had been operating under these assumptions all along, and would, in one sense rightly, claim that they had always been biotechnologists.⁷ This is a recognition of the fact that the reductionist paradigm and engineering model had always been at the core of agricultural science. No matter that the demands for and admiration of "basic" research for its own sake had always existed in botany and animal science, the assumption by agriculturalists that such basic research would provide valid manipulatory techniques for increasing food production (in a Baconian fashion) had always been present.

For all practical purposes, the massive shift to biotechnology research in agriculture was driven by absolute faith in the practical possibilities seen present in the

⁷ Numerous histories of agricultural biotechnology such as Robert Bud's The Uses of Life, 1993, try to make the case that fermentation of beer by ancient cultures was a kind of "old" biotechnology and that all agriculture fit into this same category. Another variant of the argument concerning "new" v. "old" biotechnologies deals with the comparison of tissue culture to genetic engineering.

molecular reductionism inherent in the central dogma. Its success is otherwise incomprehensible given the nearly uniform dismissal of its "immediate, practical utility" by the traditional plant and animal breeding establishment whose key "new" technologies in this period were biometrics and more complex, computer-aided statistical analysis applied to traditional crossing techniques.

It was only when genetics, plant development and plant physiology were conceptually integrated and unified in this post-world war II period, that the framework biotechnology required "emerged." The true biological revolution, especially as applied to agriculture, was a unification of biological science under the "laws" of evolution and DNA. The resistance of traditional plant scientists, and especially breeders, to the possibilities of biotechnology could never be a cogent attack on its premises, which they shared as well, but only on the nature of the complexity involved, and technological feasibility of success given the current research base. Calls to prevent research diversion into biotechnology per se were most often demands, not for holistic breeding programs, but rather for a more intensive study of the biochemical and physiological relations between genes and phenotype expression.⁸

⁸ For example R. W. Simmonds, "Plant Breeding: The State of the Art" pp. 5-25, In: Genetic Engineering of Plants: An Agricultural Perspective, 1983, states that the most important traits are polygenic and require an intense understanding of the biochemistry involved before even

Arguments were thus not over the paradigmatic acceptance of the implicit reduction of phenotype to genotype, but rather on issues of "how many genes were involved" and the exact nature of their biochemical expression as played out in physiology and development. Critics attacked the research program as ill-advised, simplistic and premature, but never dismissed the reductionistic core, for that would have been to dismiss the central dogma of biology as a modern science. Such an attack would have also undermined their own Mendelian research programs, based ultimately on the dependable link between genotype and phenotype.

One of the first major discussions of plant genetic engineering, held in California in 1983, puts these arguments into perspective. Extravagant hopes and claims by molecular biologists, research administrators, government funding agencies and industrial entrepreneurs were pragmatically countered by the breeders nearly uniform dismissal of the technology as impractical. Biotechnology, the breeders felt, was useful for basic research. They highly supported biotechnology as a useful prerequisite for obtaining reductionist knowledge of gene behavior for their

imagining to manipulate them using genetic engineering. "The oft-quoted dream of making cereals fix their own nitrogen, of turning them into legumes, so to speak, would rest on the transfer of not just one gene, but a multiplicity of them. A profound modification of the biochemical architecture of the cereal is implied." (p. 22).

own programs -- but saw it as useless for an immediate, utilitarian approach to plant improvement.⁹ Implicit on the one hand were the condemnations of naivete leveled by traditional workers against the laboratory scientists. On the other hand was the accusation by biotechnologists that field researchers and breeding practitioners simply lacked the vision of the supporters of the new technology.

How then, only five years later, and continuing to this day, did the biotechnologists "win?" By 1988, pioneers of plant breeding such as Neal F. Jensen of Cornell could declare that 1975 was the beginning of the new era and that he was writing "in the transition period between two eras, that of traditional genetics-based methods and that of biotechnical genetic engineering."¹⁰ Biotechnology was in a better position to seize resources because of its perceived, and in one sense "undeniable" potential. Because of their

⁹ Once again, R. W. Simmonds, "Plant Breeding: The State of the Art" pp. 5-25, In: Genetic Engineering of Plants: An Agricultural Perspective, 1983., states that genetic engineering "will do beautiful things but is unlikely to be more than marginally useful. Plant breeding is not about to be revolutionized." (p. 22). Equally, however, he affirms the basis of plant breeding as a science-based technology that had "long since passed the stage of being an 'art.'" (p. 5), and one which is based in evolutionary science which produces "new genotypes of superior adaptation to environments." (p. 6). It is noteworthy to see this reduction of crop plants to "genotypes" as a concept with which genetic engineers would readily agree.

¹⁰ N. F. Jensen, "Historical Perspectives on Plant Breeding Methodology," pp. 179-194. In: Frey, K. J. (ed) Historical Perspectives in Plant Science (Ames: Iowa State University Press, 1988) p. 191.

fundamental consensus on the nature of biology and the fundamental goals, the two camps (traditional vs. "biotechnic") could only war on methodological issues -- technological feasibility, timing and approach -- since both relied on the same biology to validate their ultimate research programs. And in such a war "progress" was fated to win in a field as doggedly progressive as agricultural research.

To understand the success of the newcomer against the establishment, however, it is necessary to examine the basis of the appeal. Biotechnologists could "triumph" in this competition only by convincing themselves, government and society at large that the scientific foundations and the institutional capabilities for real gains, real progress was already in place. All this had to be done at the same time as they proceeded to coopt the demands for further basic research insisted on by their foes. Biotechnologists counter-attacked with the assertion that they alone were in a position to expeditiously answer the necessary basic research questions -- and by using the new technology itself.

Ultimately, the biological revolution and the unity of biology within the biochemistry of the gene provided a basis for grandiose claims in a paradigm grounded on the achievements of the most respected sciences and provided a methodological assurance that "progress" in biology would be

accomplished as it had been for the hard (physical) sciences. As genetics provided the mechanism for evolution, so DNA provided the mechanism for genetics. Manipulation of DNA would thus provide all of the traditional benefits dreamed of from the early days of manipulating plant and animal varieties through traditional breeding as well as providing all of the benefits/possibilities that evolution itself was capable of -- which, in essence, meant all things possible to life of any sort under any conditions.

Al Wood testified to Albert Gore's Congressional Committee on the new biotechnologies in June, 1982 saying: "As far as impacts in agriculture [of biotechnology], you can just let your mind run and about anything you can imagine, I think, probably will happen in the next 10, 15, 20, 25 years. . . . there is really probably an opportunity to move, at some point in time, just about any gene or group of genes just about anywhere we would like to."¹¹

Once the possibility of DNA exchange became apparent, this unity became a functional promise of mix and match genetics and genetic engineering became the promised panacea of all present and future biological problems for medicine, agriculture and the environment. No better case for this can

¹¹ Testimony of F. A. Wood In: Potential Applications of Recombinant DNA and Genetic Engineering and the Likely Impact on Plant Sciences House of Representatives, Subcommittee on Investigations and Oversight, Committee on Science and Technology (Washington, D. C.: U. S. Government Printing Office).

be made than then by pointing out the frontpiece of the 1986 Yearbook of Agriculture entitled Research for Tomorrow.¹²

The USDA yearbooks have traditionally been one of the Department's key means of simultaneous lobbying, public-relations, self-congratulation and memory management -- being as they were compilations of current goals, historical analysis, and paeans to successful research. They routinely addressed current "hot" topics or attempted to create them. Stringing together the titles and the years of publication for these publications in the post-WWII years can be a useful exercise in determining the psychic pulse of the institution as a whole, from the 1943-47 compilation Science in Farming to the 1980 Cutting Energy Costs to the 1981 Will there be Enough Food? and onward to the 1983 Using our Natural Resources. The cover of the 1986 volume showed a human hand with thumb and forefinger pinched together at a nexus where a coiled strand of DNA unwinds to form a rainbow set of parallel colored icons of ladybird (insect - and a beneficial one, no less), hog (animal), wheat (crop plant); fir tree (forest crop), and a blue-colored water droplet. The droplet could either be representative of invisible bacteria or perhaps water quality and nature. That same hand appeared at the front of each chapter of the book, holding or manipulating a laboratory device/instrument such as forceps or slides, a DNA molecule or, in the forestry

¹² USDA, Research for Tomorrow, 1986.

section, a cross-sectioned stem -- human control through technology.

Even more significant to the assumptions of the power of the new technology in agriculture and the unifying aspects of the molecular paradigm was found in the preface by John Crowley, the Yearbook editor:

From the beginning, the choice of subjects to be included has been controversial -- perhaps inherently so. It is not easy, pleasant, or perhaps even rational, to leave out research into soil and water conservation, aquaculture, farm management, irrigation, marketing, tillage, traditional plant and animal breeding, plant and animal production, traditional pesticides, rural development, transportation and economic consequences -- to name just a few subjects in a very broad spectrum -- and still maintain that this is a book about agricultural research.¹³

But Crowley did make the choice to structure the book almost completely around biotechnology and the related biocontrol, showing its new importance compared to the old technologies.

Critical, then, to the birth of institutional biotechnology was this split between traditional plant and animal breeding techniques and their supporters and the promises of the hyper-reductionist genetic engineers. The failures of agriculture portrayed in the last chapter were assigned to the very forces responsible for the phenomenal post-war success -- traditional agricultural research. Without changing the goals, and in fact, by out-promising the same results or miraculously better ones with newer,

¹³ USDA, Research for Tomorrow, 1986. p. ii.

"cleaner" technologies, biotechnology and genetic engineering shifted the nature and definition of both basic and applied research in modern agriculture. Research funding priorities shifted in response to the promissory notes issued by the molecular biologists. For these to be credible to the point of supplanting the focus of earlier programs in funding and rhetoric, they had to provide a research foundation both in institutional settings and scientific output that was capable of stimulating and capturing the imagination of research administrators, government policy makers and the public at large. Without this base of research and rhetoric, genetic engineering and the "new" biotechnologies would have never been capable of asserting such unprecedented and rapid control of increasingly large portions of the resources available to biological research in this period at both state, federal and private levels.¹⁴

In the world of practice, plant and animal science, especially in concert with agricultural goals and institutions, had, in the post-DNA world, created a battery of scientific tools and examples that not only confirmed for them the central dogma, but confirmed also the grandiose and as yet unfulfilled possibilities that the unity of life

¹⁴ As Chapter 8 will show, biotechnology was phenomenally successful in attracting funding away from its traditional competitors, capitalizing on its cachet as being more scientific, but also on the glamour of its industrial ties and Wall Street hype in a world of increasingly competitive markets.

promised. These tools arose independently of the technology of recombinant DNA (which has been endlessly reviewed and commented upon elsewhere)¹⁵ and provided the receptacle in which rDNA techniques could best be transplanted from microbiology and survive.

These techniques would be seen as basic research tools by biologists in general> But to the "applied" biologies of medicine and agriculture, they would be seen as the ultimate sources of manipulative power and the potential solution to problems as old as humankind. These developments were what made it logical to ignore the "traditional" topics of plant and animal breeding, pesticides, etc. when reporting on "Research for Tomorrow."

The Science Behind Agricultural Biotechnology

Genetic engineering and the new biotechnologies were not embraced in a vacuum. New research programs are not easily declared by executive fiat or administrative "brute force." Even something as politically and societally driven

¹⁵ For a documentary history of the rise of recombinant DNA technology see: J. D. Watson and J. Tooze, The DNA Story: A Documentary History of Gene Cloning (San Francisco: W. H. Freeman and Company, 1981). For a business history of the technology see: R. Teitelman, Gene Dreams: Wall Street, Academia and the Rise of Biotechnology, 1989. For a cultural history see: D. Nelkin and S. Lindee, The DNA Mystique: The Gene as Cultural Icon (New York: W. H. Freeman and Company, 1995). For a political history, see: S. Wright, Molecular Politics (Chicago: University of Chicago Press, 1994).

as the Manhattan project required a theoretical foundation and trained personnel in advance, and was still much delayed by the necessary development of a technological infrastructure. The rapid "emergence" of biotechnology in agriculture, as in biology as a whole, was the result of an evolution from previous disciplinary and institutional programs and commitments.

In the early to mid-1970s such disciplinary and institutional commitments in agriculture seemed to be structured around three main "sciences" or six main disciplines/programs: 1) Plant and Animal Pathology, 2) Plant and Animal Physiology, and 3) Plant and Animal Genetics/Breeding. These were usually institutionally separate programs -- the divide between botany and zoology was still a reality at many institutions, especially traditional land grant colleges such as Michigan State and the University of Florida. But cross-disciplinary programs and mutually overlapping research mentalities and training had removed much of the surface differentials such that the molecular rubric could be used as a unifying standard for the creation (or attempted creation) of generalized "biological science" programs throughout the country, including the University of Florida (a move highly enhanced as a result of both the evolutionary synthesis and the development of the DNA central dogma).

One component of these unificatory moves was funding

exigencies. In a confidential memorandum to E.T. York on his visit to the National Science Foundation on September 4, 1969. L. E. Grinter states: "What was made eminently clear was that a superficial or loose reorganization of the biological sciences would carry little weight when evaluated by NSF committees. Only a strong, clear-cut plan to provide real unification of the fundamental biological sciences and improved opportunity for disciplinary cooperation throughout the University would carry weight."¹⁶ In an interesting sidelight -- the proposed administrative unification under discussion at UF was opposed by many in IFAS, not for scientific, but for professional reasons. Agriculture seemed to be slighted in the plan: "Why should the School of Biological Sciences be placed in the College of Arts and Sciences when the majority of the Biological Scientists are located in IFAS?"¹⁷ Because of such disciplinary conflicts, and despite the scientific impulses leading to unity, the proposed School of Biological Sciences was never implemented.

It is not my intention here to argue for a strong

¹⁶ Memorandum to E.T. York on his visit to the National Science Foundation on September 4, 1969. from L. E. Grinter (Gainesville: University of Florida Archives).

¹⁷ A. H. Krezdorn, May 27, 1969. Letter to Department Chairmen and Administrators in IFAS. Subject: Committee Action Regarding the Plans Submitted for the Formation of a School of Biological Science in the College of Arts and Sciences. (Gainesville: University of Florida Archives, p. 3.).

unification of the plant and animal versions of these disciplines at this point, I merely wish to argue that methodologically and institutionally, for both plants and animals, these three scientific disciplines provided equivalent foundations for the evolution of research programs into biotechnological directions in agriculture.

Plant and Animal Physiology

As was obvious from the earliest days of agricultural chemistry, plants could be thought of as conglomerates of chemicals which could be analyzed to their components and, with intuitive simplicity, reconstructed by the addition of fertilizers to soils that had ceased to be productive. Thus, from the very first, soil analysis/diagnosis and the development and application of new fertilizers were seen as key contributions that applied agricultural science could provide. Agricultural plant physiology was thus born of plant growth and nutrition studies.

Perhaps expectedly, the field of animal physiology and nutrition in agriculture was a direct outgrowth of the recognition that animals could be considered in some way as biochemical conglomerates of what they ate, as plants were compilations of what they took from the soil or produced from the air. From the earliest days of animal experimentation, the USDA developed concepts of nutrition and animal health and well-being based upon chemical feed

analysis, analysis of growth parameters and tissue type (fat or protein), and even excrement analysis (both fecal and gaseous, including carbon dioxide). It was a physiology at first based almost exclusively on nutrition, only later was a nod given to genetics when comparisons were initiated between breeds and families. A further sign of biological unification was the turn of the century experimentation at USDA on human nutrition that resulted in young men being placed in large animal calorimeters for identical patterns of analysis as pigs, sheep and cattle.¹⁸

In the modern era, concerns for human and animal nutrition were directly influenced by physiological determinations of balanced requirements for vitamins and proteins based on destructive analysis of plant and animal food sources. The quest for high-lysine corn was one of the key results of such a mentality and provided a model for the possible fruits of latter-day genetic engineering -- a goal for which traditional breeding seemed inadequate.

In this well-known example of nutritional sociology, it was demonstrated that corn is low in the amino acid lysine. Lysine was high in beans, which lacked the caloric content of corn. As the human dietary tract required the presence of all amino acids in a particular balance in order to efficiently utilize any of them, lack of lysine made corn a

¹⁸ See: A. C. True and V. A. Clark, The Agricultural Experiment Stations of the United States, 1900. pp. 69-71.

nutritionally poor diet for humans when eaten alone. Stories based upon the nutritional soundness of pre-industrial peoples pointed out the development of the traditional corn/bean meals of Central and South American natives as being a response to such nutritional requirements -- the bean stuffed tortilla, in a peculiar way, being the first "health food." Repair of such nutritional inadequacies became a prominent goal of researchers determined to provide aid to third world nations whether from humanitarian, pragmatic or cold-war political aims.¹⁹

Biological Nitrogen Fixation

Overall, researchers wedded to such projects provided the institutional and educational wherewithal to pursue such goals with the "new" biotechnologies. The issue of plant proteins, due to the critical nature of protein deficiency as well as the apparent lack of general caloric intake in third world nations in this period was a tremendous spur to such research programs. This was well before the advent of the "new" biotechnologies and provided both the scientific and institutional requirements for a move in that direction. Biological nitrogen fixation appeared to be the potential natural panacea to these problems and was one of the key research enterprises of scientific agriculture in this

¹⁹ See discussions in Palmer, Food and the New Agricultural Technology, 1972. and Brown, By Bread Alone, 1974, pp. 164-166.

period. Robert Burris outlines historical developments in biological nitrogen fixation, pointing out that the Frenchman, Boosingault, provided in 1893 the first evidence that legumes fixed nitrogen.²⁰

In the earliest days of Greek and Roman agriculture known, the idea of crop rotation was used to help soils recover from the obvious ability of most major crop plants, especially grains, to deplete their fields and lose productivity over the years. Legumes seemed to be the ideal fallow crop, although demonstrating correctly the nature of their nitrogen-fixing abilities proved to be one of the earliest failures of the famous British Rothamstead experiment station. Careful sterilization of the soil used before the experiment deprived the legumes of their requisite bacterial partners, and no fixation could be documented. In one of the standard demonstrations of scientific nationalism at the time, the French re-investigated and stood by their colleagues. Significantly, in the United States, W. O. Atwater, the first director of the Office of Experiment Stations, questioned the Rothamsted accounts as well and pushed the idea of nitrogen fixation by leguminous plants. After definitive experiments by Atwater by 1888, the system was considered as a possible solution to one of the key problems of scientific agriculture: that of

²⁰ R. H. Burris, "Historical Developments in Biological Nitrogen Fixation," pp. 2-41., In: Frey, K. (ed) Historical Perspectives in Plant Science, 1994.

poor soils and the requisite costs and difficulties of fertilization.²¹

In this century, the discovery of nitrogen fixing bacteria and the symbiotic link between them and their hosts provided one of the strongest boosts to the application of microbial genetics to agricultural studies. It provided one of the first opportunities for the development of an agricultural biotechnology "industry" on lines much like the one-product venture capital companies of the very modern era -- companies arose to develop and sell legume inoculant bacteria.²² Inoculum production equally provided another of the early opportunities for government regulation when such companies proved unscrupulous or lax to the consumer. Scientifically, as well as commercially, the realization that some microbes were more efficient than others at nitrogen fixation, that some preferred different soil types or host organisms to others gave evidence of a possible selectable variation that promised much for the replacement of fertilizer dependency in the post-world war II era. From the early 1900s on, the nitrogen-fixing bacteria and allied microbes provided a stimulus to voluminous biochemical studies of enzyme pathways, genetic analysis of host/symbiont interactions and a world-wide quest for new

²¹ R. H. Burris, Historical Developments in Biological Nitrogen Fixation, 1994. pp. 24-27.

²² Norman, Organic Matter in Soils, pp. 509-510, In USDA, Science in Farming, 1947.

and better symbionts.

If the intrinsic benefits to self-fertilization were not enough, the oil crisis in the 1970s drove up the costs of manufactured fertilizers. This damaged home production and endangering the "Green Revolution" abroad. When coupled to the newly arising public outcry over pollution caused by farm fertilization, the drive for increased use of biological nitrogen fixation provided sufficient impetus to turn this field into one of the first of the "Big Science" biology projects undertaken in the modern era.

Significantly, biological nitrogen fixation was one of the major areas of research designated fundable under the newly instituted Competitive Research Grants Program funded by congress and administered through the Cooperative State Research Service of USDA in 1977. It was also, as described by Mary Clutter, representing NSF grants, at the 1983 meeting: "Genetic Engineering in Plants" one of the major areas of plant research considered fundamental or "cutting edge" science by the agency and thus worthy of funding, accounting for 14, 15, and 12 percent of research award dollars in 1977, 1979 and 1981 respectively.²³ (Significantly, in those same years, the category "Genetic Manipulation" leaped from two to seven to 13 percent --

²³ Remarks of Mary E. Clutter, pp. 468-470 In: "A Roundtable Discussion on Research Priorities," pp.467-486, In: Kosuge et al. (eds) Genetic Engineering of Plants: An Agricultural Perspective, 1983.

again, well before successful gene transfer of a non-Agrobacterium gene into a plant system). Such impetus, coupled to the firm support of US-AID under the auspices of a continuing "Food for Peace" program provided not only scientific validation of biological nitrogen fixation as a desirable research area, but emphasized the humanitarian and patriotic aspects of such research. To the U.S. government, it was needed to stabilize a third world caught in the grip of apparent Malthusian forces that set it as a potential victim of Marxist propaganda and Soviet or Chinese dominance.²⁴ In this way, the Cold War was the final arbiter of all American foreign policy, including agricultural policy, that affected global markets or international productivity.

The expanding area of biological nitrogen fixation provided both the trained scientists and the scientific mindset to prepare for the coming wave of microbial based genetic engineering in agriculture.²⁵ And most significantly

²⁴ R. G. Stanley, Food for Peace: Hope and Reality of U. S. Food Aid (New York: Gordon and Breach) and D. E. Shaughnessy, "U. S. Agriculture and World Security" pp. 42-48. In: Will There Be Enough Food: 1981 USDA Yearbook, (Washington D. C.: USDA, 1981).

²⁵ The rhizobium genes used in nitrogen fixation would be some of the first used for genetic engineering studies. More importantly, the possibility that the genes involved in nodule formation were similar to those in Agrobacterium provided not only another potential "natural" vectoring system, but an ideal study model for gene integration with agronomically important potential. (They also provided an excellent example of host/parasite interactions that would spur molecular plant pathology research. M.J. Daniels in the

from a research administrative point of view, nitrogen-fixation scenarios would provide one of the first of the captivating models of potential biotechnology miracles -- that of nitrogen-fixing grasses. Self-fertilizing wheat or corn became a "staple" of rhetorical flourish ever after. Significantly, it was Iris Martin, associate program manager of the Competitive Research Grants Office, who reported in the 1986 Yearbook on Agriculture on 10 years of "tremendous advances" in understanding the molecular basis of nitrogen fixation, and reported on its applicability to non-leguminous plants. This research was funded in a significant measure part by Cooperative State Research Service (CSRS) grants.²⁶

Researchers and administrators at the University of Florida, for example, were much affected by the promise of biological nitrogen fixation. It also provided a bridge to the new biotechnologies for several reasons. First, there was microbial genetic manipulation. As microbes were the first and originally the only biological entities capable of genetic transformation, the existence of these species of useful bacteria and related organisms provided hope for the earliest use of genetic engineering technology. The earliest

1988 meeting on the Molecular Genetics of Plant-Microbe Interactions details how such research lead in the early 1980s to molecular genetic tools and models easily adapted to plant pathogen studies).

²⁶ Martin, Nitrogen Fixation in Non-Leguminous Plants, pp. 112-116, In USDA, Research for Tomorrow, 1986.

conferences on plant genetic engineering dealt with the modification of nitrogen fixing bacteria.

Secondly, there was host genetic selection. Attempting to match host with microbe, especially when there was a possibility of engineering that microbe with variant traits provided a ready made possibility for biological optimization in an area critical to solving the eternal fertilizer quest on which the agricultural chemistry paradigm was founded

Finally, there were nitrogen fixing grasses. The association of some tropical grasses with nitrogen fixing organisms, given the conviction of the unity of life and of life processes and the ability of, at least, microbial genetic engineering to work, provided hope for the development of the ever elusive, ever-promised nitrogen fixing corn. The University of Florida played a significant role in developing this research area.

As far back as 1967, the University of Florida was participating with Brazilian scientists, especially Johanna Dobreiner who obtained strains of grass from the U.F. campus to use as part of her testing on expanding the plants on which nitrogen fixing bacteria could be grown.²⁷ This was well before the genetic engineering triumph of reductionism in the 1970s when the *nif* (nitrogen-fixation) genes were

²⁷ Institute of Food and Agricultural Sciences Press Release, 1975. University of Florida archives.

transferred from one bacterium to another, along with their ability to fix nitrogen. But it created a mindset in researchers and administrators and a rhetoric linking the benefits of biological nitrogen fixation to cereal production not only in terms of productivity benefits, but with regard to pollution control as well.

In June, 1974, the Chairman of the Department of Agronomy at the University of Florida was touting the discovery of a nitrogen fixing bacterium associated with Brazilian Transvala digitgrass. "In today's energy expensive and fertilizer-nitrogen short world, this could prove to be one of the most important new discoveries in the agricultural field." and tellingly: "While it is as yet premature, one of man's long time dreams "a nitrogen fixing grass" may become a reality."²⁸

By December 1974, the Technical Assistance Bureau of USAID was soliciting proposals from the University of Florida's International Program's office for research funds to work on nitrogen fixation and the National Academy of Sciences was supplying travel funds to Dr. Shirley West, administrative head of the University of Florida's Nitrogen Fixing working group for visiting Dr. Dobereiner in

²⁸ Letter from D.E. McCloud, Chairman Agronomy Dept. to K. R. Tefertiller, Vice President for Agricultural Affairs. June 7, 1974. Subject: New Findings on Symbiotic Nitrogen Fixation of Grasses.

Brazil²⁹.

In January 1975, Leon Hesser, Acting Director of the Office of Agriculture, Technical Assistance Bureau of AID was writing to beg off funding the submitted research proposals for two to three months. "Because the problem is so important, and improved biological fixation of nitrogen holds so much promise for developing countries, we have concluded that we need to develop a position paper on the subject."³⁰

By 1975 Biological Nitrogen Fixation had become one of the key research areas in the Agronomy Dept. of the University of Florida's Workgroup on Grasses and Forages. They described the situation thusly: "Eliminating hunger and malnutrition in underdeveloped areas is impeded by the rapidly increasing cost of chemical fertilizers. Greater use of symbiotic nitrogen fixation is one of the most promising avenues for increasing production of food and feed crops. Recent research results indicate that certain bacterial species, living in the root zones of some tropical grass species, can fix substantial quantities of atmospheric

²⁹ Letter from Shirley West, Assistant Dean for Research to Michael McDonald Dow, Deputy Director, Board on Science and Technology for International Development, National Academy of Sciences. December 6, 1974.

³⁰ Letter from Leon Hasser, Acting Director, Office of Agriculture, Technical Assistance Bureau, Agency for International Development, to Hugh Popenoe, Director, International Programs, Center for Tropical Agriculture, University of Florida, dated January 30, 1975.

nitrogen.³¹"

Of particular interest however, was the marshalling of environmental rhetoric as well as energy efficiency, especially in public pronouncements as in this 1975 press release from the Institute of Food and Agricultural Sciences at the University of Florida:

In research that could notably affect world food supplies, scientists are now experimenting with special bacteria-treated tropical grasses which convert nitrogen from the air into nitrogen fertilizer for their own or use for other plants.

The research ... is an effort to biologically transform the unlimited reserves of the atmosphere into non-polluting, low-cost fertilizer to aid food production.

Based on currently laboratory yields of nitrogen in certain strains of treated grass, widespread application would possibly halve the world's need for man-made nitrogen fertilizer.

Such a reduction in man-made nitrogen fertilizer would mean a drastic reduction in oil consumption. Currently seven barrels of oil are required to produce one ton of ammonium nitrate (nitrogen fertilizer) -- and with the world energy crisis the price of manufactured fertilizer has more than doubled during the last year.

The press release goes on to proclaim how the National Academy of Science and the Brazilian Research Council had set up a research committee of six plant and soil scientists, including Shirley West, the Assistant Dean of Research at IFAS, to work with Dr. Dobereiner following a paper she presented to the Academy. This research was designed to pair grass and bacteria to discover the best

³¹ Memorandum to Dean Davis, from G. O. Matt. Subject: Revised Copy of Report from Workgroup on Grasses and Forages.

combinations for high nitrogen yields.

With the standard optimism the report declares : "Any widespread application would be three to five years away."³²

The Southern Directors of the Agricultural Experiment Stations authorized a "Nitrogen Fixation in Grasses Group" with Dr. West as Administrative Advisor at their spring 1975 meeting, indicating broad spectrum appreciation of its potential at the time. The 1975 Progress report and 1976-1977 budget proposals for Nitrogen Fixation submitted by West to IFAS indicates USAID and Agricultural Research Service (ARS) support for the work with grasses including funding for additional laboratory technicians.

In an internal report to IFAS administrators in 1977, tissue culture experts Vilma and Indra Vasil working in conjunction with nitrogen fixation researcher David Hubbell, expanded on work done in 1975 with Rhizobium in tissue culture which showed that the bacteria could fix nitrogen when grown in association with tissue cultures of both legumes and non-legumes. Vasil et al. reported that "callus cultures of sugarcane inoculated with Spirillum continue to grow on media either completely devoid of added nitrogen or supplied with very low levels of nitrogen. ... Our data strongly indicate that the nitrogen needed by the sugarcane tissues for growth is being supplied by the bacteria."

³² IFAS Press Release, 1975, University of Florida archives.

Success or failure of such programs is not the issue, what is apparent is the consistency of the motivation and rhetoric used to sell the program and how biological fixation in cereals had developed a firm hold as a potential agronomic miracle well before genetic engineering technology became applicable to plants. (The first recombinant DNA transformed plants were not produced until the early 1980s).

Hormone Research

Among the earliest of the new naturally occurring chemical panaceas were the plant hormones characterized and utilized at this time. Not only did they provide an path to an entirely different form of chemical industry, cadres of scientists and even institutional structures (such as the Department of Energy - Plant Research Laboratory at Michigan State University, for example), but they created a mentality of chemical control at a much subtler and specific level of development, preparing the mindset for single gene control. Their pleiotropic vs. specific effects prepared scientists for the concept of multi-gene interactions and the possibility of specific physiological uniqueness in the time and space of tissues in an individual plant.

Explication of hormone response became a key question for the new biotechnological approaches and provided a model for expecting differential gene expression or effects in different physiological (in the same plant over time and

tissue) or genetic environments. Quests for the elusive flowering hormone "florigen" were instituted. Gibberellins had been discovered by the Japanese in 1926. They were first found produced by an infecting fungus that stimulated faster and taller growth compared to that in "healthy" plants. Eventually these were found as types of naturally occurring compounds. Along with auxins and cytokinins,³³ the two other major classes of plant hormones, they promoted the idea of chemical regulation of development and of potentially important agronomic traits. Plant hormones served as an ideal candidate for a chemical messenger from the gene able to instruct the organism to produce an appropriate phenotype.

Plant hormone research led to the development of major new herbicides in the arsenal of chemical control (including defoliants such as Agent Orange used in the Vietnam War). Such research also provided the developing field of plant tissue culture with the regulatory compounds needed to both study and promote cell and tissue development and plant

³³ It is interesting to note how closely tied the history of auxins and cytokinins are tied to tissue culture research. As detailed by R. J. Guatheret in his "History of Plant Tissue and Cell Culture," pp. 2-59, In: Vasil, I. K., (ed) Cell Culture and Somatic Cell Genetic of Plants, Vol 2 (New York: Academic Press, 1985), the auxin, indole acetic acid, discovered in 1926, was found to stimulate callus development, and cytokinin was discovered as the active ingredient in the culture amendment, coconut milk, which allowed for increased cell division in callus. These hormones were critical to the full development of tissue culture as a viable tool for biotechnology and genetic engineering.

regeneration. Plant hormones provided a key link in the "physiological genetics" chain required to make convincing an argument that gene control was phenotype control and that the process was amenable to biochemical deconstruction. Genetic control of hormone regulation would provide an enticing promise for the agronomically important results that biotechnology might one day produce .

In addition, the birth control industry relied upon the biochemical similarity of plant and animal compounds. The development of synthetic animal hormones was ultimately derived from knowledge of plant steroids. These steroids were originally obtained from yams and processed in Mexican pharmaceutical "factories", replacing early, uneconomic and impractical, attempts to purify sufficient quantities from animals. Manipulation of the female reproductive system biochemically became another example of successful biological reductionism and the overlap of plants and animals as molecular entities.³⁴

Another aspect of hormone research being commercialized at this time was the burgeoning ag-chemical/herbicide

³⁴ Bud discusses one of the links between the development of birth control in molecular biology in his The Uses of Life, 1993. Lederberg, one of the pioneers of molecular biology and the vision of biochemical genetics, was given a lab at Syntex, the commercial developer of synthetic progesterone. He was convinced that understanding of hormones would help "to develop better methods of controlling fertility in humans, animals and plants." They were, in the 1960s envisioning gene transfer "of genetic material between higher organisms." (p. 171).

industry. "Spray and pray" studies of artificially synthesized compounds were coupled to a more intimate physiological appreciation of hormone activity and provided a feedback loop to the study of plant metabolism. Due to the effects of many of these compounds, they also provided a number of powerful research tools for regeneration studies and studies in reproductive behavior of both plants and insects. The discovery of natural analogues of the artificially constructed herbicides, insecticides and hormones provided a further excuse for a "nothing new under the sun" attitude, but also a greater link to ideas of biological unity and physical reductionism in genetic interactions and evolution.³⁵

On a more mundane level, off-the shelf hormones provided the molecular tools to manipulate cells and tissues in the laboratory to "perform" in appropriate ways -- whether as cell factories or transgenic regenerates.

The intimate relationship between Agrobacterium, the genetic engineering vector that came into prominence as the best vehicle for genetic engineering, and hormone genes provided an additional tie between hormone research, genetic

³⁵ For a discussion of auxin related artificial herbicides and the discovery of the natural auxins see: T. C. Moore, Biochemistry and Physiology of Plant Hormones, (New York: Springer-Verlag, 1979), p. 39-44. For a series of articles on some of the many naturally produced plant pesticides see: W. J. Mattson, J. Leveux, and Bernard Dagan, (eds) Mechanisms of Woody Plant Defenses Against Insects: Search for a Pattern (New York: Springer-Verlag, 1988).

engineering and plant regeneration. This relationship linked research techniques, facilities and researchers. In nature, Agrobacterium's tumor-inducing genes were actually genes that inserted into the host and caused production of plant hormones in excess of normal cellular levels, stimulating a "cancerous" uninhibited cell division and leading to what was defined as a plant "tumor." Originally, tumor production was the selectable marker by which transformed cells were isolated.³⁶

The cross utilization and production of all these compounds, and the ability to make industrial analogues, was another key feature that would allow scientists and administrators to assume that the new biotechnologies might prove pertinent in the agricultural world well before scientific "proof of concept" had been demonstrated.

³⁶ The appropriation of medical/animal terminology to describe plant phenomena was pervasive throughout the period, despite the definitional sloppiness -- plant hormones are not definitionally hormones in that they are not proteins. Their specificities (at least of the major categories) are far less restricted and their active timing and quantities far different from the behavior of animal hormones. However, they appeared to serve the analogous function for plants and the same genetic regulatory "niche" hence the terminology was retained. A similar borrowing occurred for words such as "tumor" and toxins. Such borrowing and its assumed validity was another evidence of the conceptual unity among the biological sciences. Whether it was animal centrism or Occam's razor, there seemed to be continued expectations of biochemical uniformity in control and behavior across kingdom lines, which indicated reductionistic assumptions based on some sense of the unity of life arising well before the central dogma had become fully articulated.

Tissue Culture

Plant and animal tissue culture continuously fought side by side with recombinant DNA technology against "traditional" agricultural research for pre-eminence as a new biotechnology. They were both ancillary to the development of genetic engineering (as defined in its narrow sense of rDNA transfer). Both dominated agricultural "cutting edge" research from the earliest days as fundamental products of the biological revolution. Hormone research blended with tissue culture to provide some of the most cogent precursors to plant genetic engineering at least a decade before the utility of recombinant DNA became evident for higher organisms. In the 1970s massive cloning methodologies became possible, and with the development of protoplast and somatic hybridization techniques, it seemed that tissue culture would become the dominant new technology for rescuing agriculture from the problems outlined in the last chapter.³⁷

Even more critical to ultimate ideas of developmental

³⁷ B. Hileman, "Views Differ Sharply Over Benefits, Risks of Agricultural Biotechnology," Chemical & Engineering News August 21, 1995: pp 8-17, discusses the aspects of tissue culture as a panacea where genetically altered crops would be produced to "increase yield, eliminate the use of several insecticides now derived from fossil fuels. . . reduce health risks and groundwater contamination. . . decrease the amount of herbicide used and allow for no-till agriculture, which can minimize erosion." (p. 15). Of course, each of these aspects has a damaging "flip" side, but that was not at issue to the biotechnology visionaries.

biology and the use of tissue culture was the early indication, provided by cells treated with Agrobacterium cultures, of plant totipotency -- the demonstration that a single somatic cell could regenerate into a completely fertile plant. This notion, now so obvious, was an important proof of concept experiment for tissue culture. It proved that the genetic material in the nucleus of every cell contained the information necessary to recreate the entire organism. Any uniqueness of fertilized eggs was not at the gene level or due to a peculiar form of sexual vitalism. If genetic transformation of higher organisms were not to be limited to manipulation of germplasm, the system required the ability to grow somatic (and genetically engineered somatic cells at that) to form whole plants.³⁸

The idea of the totipotency of individual cells lead to the validity of attempting to regenerate whole plants from protoplasts (plant cells from which the cell wall had been removed) and the entire protoplast/somatic

³⁸ It was not until the 1960s that plant tissue culture solved the problem of demonstrating totipotency -- the critical requirement for "cloning" to be a theoretical possibility. For the first time, V. Vasil and A. C. Hildebrandt, "Differentiation of Tobacco Plants from Single Isolated Cells in Microcultures" Science (1965) 150:889-890, demonstrated that isolated cells of a tobacco hybrid could be grown into complete plantlets. For an in depth discussion from this period, see: V. Raghavan, 1977. Totipotency of Plant Cells: Evidence for the Totipotency of Male Germ Cells from Anther and Pollen Culture Studies. p. 155-178, In: Sharp, W. R., Larsen, P. O., Paddock, E. F., and Raghavan, V. Plant Cell and Tissue Culture Principles and Applications. Columbus: Ohio State University Press.

hybridization research program. The first movements in protoplast work in the modern era were in England and Japan. Japanese researchers, A. Nagata and I. Takebe in 1971 were the first to regenerate plants (tobacco) from protoplasts. Commenting on the importance of this research, R. J. Gautheret states that: "This result was verified very quickly by many people who suddenly rushed to this field of research."³⁹

Institutionally, the Department of Energy/Plant Research Laboratory (DOE/PRL) at Michigan State University was a critical site for this type of plant research in the United States. The work of Peter Carlson at this U. S. laboratory, combined with the work of Olaf Gamborg in Canada and others, helped transform protoplast fusion from a curiosity to one of the original sources of biotechnological "hype" that paved the way for the massive shift to the new technologies and ultimately the whole-hearted embrace of genetic engineering as the dominant "research for tomorrow."⁴⁰ To neglect the major role this technology played in creating the research programs, institutions and mindsets for embracing the next generation of biotechnology

³⁹ Gautheret, R. J. History of Plant Tissue and Cell Culture, 1985. p.47.

⁴⁰ For a scientific review and discussion of the perceived importance of protoplast fusion at the time see: Evans, D. A. and Flick, C. E. "Protoplast Fusion: Agricultural Applications of Somatic Hybrid Plants," pp. 271-288, In: Kosuge et al. (eds) Genetic Engineering of Plants: An Agricultural Perspective, 1983.

is to miss much of the preparatory scientific and institutional platform that made the 1986 Yearbook possible.

The rationale between the need for "somatic hybridization" as protoplast fusion was called, was one that would be used as the ultimate rationale for all gene-transfer techniques in agricultural systems. The "yield plateaus" discussed in the previous chapter and the narrow gene-base of most agricultural crop species appeared to have severe long-term repercussions. Attempts to incorporate new germplasm from wild relatives or wide-crosses between unrelated species were proving difficult or impossible using "conventional" methodology. Researchers hoped fusion of "somatic," (i.e., non-sexual), cells would provide the solution -- if the hybrid cell could be regenerated into whole plants and if the resulting hybrid with the desirable trait or trait in question could be easily selected for.

Normally all plant cells are surrounded by a cell wall which must be removed before cells can be fused together. Japanese researchers discovered that certain plant-pathogenic fungi produced wall-degrading enzymes which could be purified and utilized to digest away this outer coat, leaving a viable, spherical, membrane-enclosed sac called a protoplast -- the body of the cell minus its fibrous container. Because of osmotic pressures, the protoplast must always be maintained in an appropriate sugar solution to survive. When two such protoplasts were placed in close

proximity, under certain conditions (and especially with certain chemical facilitators present in the medium) the cell membranes touched and (like two drops of oil) the protoplasts fused to make one large cell. In a truly successful fusion, the two parental cell nuclei (the nucleus is the computer brain of every living cell and contains the DNA genetic blueprint) themselves coalesced and the resulting cell was an authentic hybrid.⁴¹

By the late 1970s/early 1980s, many such hybrid cell lines had been made. Some included those within the same species (tobacco + tobacco); some were between two different but somewhat related species (tomato + potato); and some between highly unrelated species (soybean + barley). Some even crossed kingdom lines (tobacco and carrot cells were fused with human cancer cells).⁴² Regeneration of whole plants was limited to the first two classes of hybrids, but as research tools and, to steal a phrase from Richard Goldschmidt, "hopeful monsters" the intergeneric crosses

⁴¹ According to Gautheret, History of Plant Tissue Culture, 1985, p. 46-48, the first protoplast fusion was achieved by the German researcher, E. Kuster in 1909. Even in this early period, Kuster was convinced that "in the future, hybridization by fusion of protoplasts would become possible."

⁴² Routine fusions across genus boundaries was achieved by 1974 in Olaf Gamborg's laboratory and others through the use of various chemical amendments including calcium salts and polyethylene glycol. (See: K. N. Kao, F. Constable, M. R. Michayluk, and O. L. Gamborg, "Plant Protoplast Fusion and Growth of Intergeneric Hybrid Cells." Planta 120: 215-227, 1974).

were especially important symbols both scientifically and culturally of the powerful abilities of laboratory techniques to perturb genetic systems in ways completely beyond traditional breeding. At the same time protoplast fusion demonstrated even more coherently the unity of life in ways more viscerally powerful than simple notions of similarity in molecular design.⁴³

A key aspect of somatic hybridization technology that helped broker increased industrial-university partnerships that dominated many of the future developments of the "new" biotechnologies was the aforementioned need of a selection system -- some way of either identifying, or, ideally, causing to preferentially survive, the desirable hybrid. In 1972, Peter Carlson reported the first successful plant protoplast fusion able to produce auxin autotrophic (i.e.,

⁴³ Distant fusion products such as produced in plants by Gamburg and in animal cell cultures routinely were seen as an early method of gene mapping and chromosome assignments. Henry Harris, details in The Cells of the Body: A History of Somatic Cell Genetics (Cold Spring Harbor: Cold Spring Harbor University Press, 1995) pp. 130-133, how heterokaryons (fused cell lines differing at the genus level) were used throughout the 1970s. Many of the earliest mapped human genes, in fact, were analyzed in human x hamster cell lines. Wide crosses tended to eliminate excess chromosomes, usually of one species or the other until a stable level was reached. It was then possible to study cell lines with unique genetic configurations to examine gene expression and interaction in ways impossible to conventional breeding or analysis techniques. The viability of the cell lines and the totipotency concept created fields for the wildest imaginative speculations as well as expanding the possible science.

hormone self-sufficient) somatic hybrids.⁴⁴ Here the selection method involved choosing parental lines each with a separate mutation interfering with its ability to produce its own auxin in culture. The hybrid alone was capable of surviving without auxin in the selection medium because it had one good gene from the alternate parent for each of the mutations. The method worked to produce the first such regenerates, and Carlson's work provide and important "proof of concept" demonstration. But unfortunately, this selection methodology was much too limited for general applicability.

The most functional genes to use for such selection methodology proved to be antibiotic resistance genes. Cells of plants which were resistant to a certain antibiotic could grow in the presence of the toxic metabolite whereas others could not. If two different cell lines, each with a resistance to a separate antibiotic, were fused, only successful hybrid cells would be capable of growing in a medium containing both. But equally useful and of interest to corporate funding sources were the herbicide resistance genes. Instead of resistance to commercially meaningless antibiotics, selection methods used resistance to herbicides of agro-industrial importance. Many of the key herbicides such as atrazine attacked plant chloroplasts (semi-

⁴⁴ P. S. Carlson, H. H. Smith, and R. D. Dearing, "Parasexual Interspecific Plant Hybridization," Proceedings of the National Academy of Sciences, U.S.A. 69: 2292-2294, 1972.

independent organelles in plant cells with their own DNA system).⁴⁵

Weed species with resistance to atrazine had appeared in fields in which the herbicide was heavily used. Companies such as Ciba-Geigy (which produced atrazine commercially) were funding researchers at the Michigan State University lab of Kenneth Sink to use protoplast and cytoplasm fusion techniques to perform the proof of concept research that atrazine resistance could be transferred by cell fusion techniques that transferred the weed chloroplasts stably into crop plant species cells. This was a post-doctoral project on which the author was employed from 1982-1984. The long term potential of such herbicide resistance being introduced into a crop species was to permit utilization of the herbicide in a broader spectrum of farm operations -- either by increasing the number of crops with which it could be used as a weed-killer (without killing the crop) or extending the time frame during which it could be used. Many herbicides could only be used on prepared fields before the crop species was actually planted, or at least before the seed had sprouted.⁴⁶

⁴⁵ R. B. Lyon, "Engineering Microbial Herbicide Detoxification Genes in Higher Plants," pp. 80-108. In: Dennis, E. S., and Llewellyn, D. J. (eds) Plant Gene Research: Molecular Approaches to Crop Improvement (New York: Springer-Verlag, 1991).

⁴⁶ Busch et al. Plants, Power and Profit, 1991 pp. 7-8 and 171, points out the enormous commercial implications of such herbicide tolerance to the companies which produce them

Somatic hybridization techniques were never seen as optimal, however. Well before the development of Agrobacterium as a plausible, efficient vector for genetic engineering, plant tissue culture scientists working in agricultural institutions were promoting the use of protoplasts for direct DNA and chromosomal uptake both as a research tool and as a method for the transfer of agronomically important traits. This direct transfer of selected genetic material was determined to be a potentially better approach than somatic cell hybridization which had the unwanted side-effects of doubling genome size and/or providing large numbers of unwanted or unselected genes to the product.⁴⁷ Peter Carlson and his students pioneered many of these approaches, adapting methods originally designed for plant virus uptake. One of his students, Robert Griesbach, went directly to the main laboratory of USDA to seek to introduce and implement this technology at the heart of the agricultural research establishment in Beltsville. Griesbach and Malmberg (another Carlson student) were invited speakers at the first "Genetic Engineering of Plants: An Agricultural Perspective" meeting in 1983, even as their technology was being supplanted by the development

and the financial ties such corporations have with the researchers involved.

⁴⁷ Unlike sex cells, somatic cells were diploid and hence hybrids contained twice the normal number of chromosomes than a normal sexual cross.

of Agrobacterium vector systems.⁴⁸

Another major research program was devoted to the infection of protoplasts and tissue cultures with plant viruses. This provided not only another possibility of developing a biological vector for gene transfer, before, and later as a potential competitor with Agrobacterium, but also helped to develop the research tools and expertise required to analyze foreign DNA and RNA in plant cells. Nucleic acid purification techniques and the use of radioisotopes to optimize and follow introduced genetic material, were developed in such research, as were many of the gel and protein banding technologies and biological assays required to confirm successful gene transfer. These were the very technologies necessary to move into the new genetic engineering that became possible with the introduction of recombinant DNA technology and improved Agrobacterium vectors.

This research began in earnest late in the 1960s when Japanese researchers Aoki and Takebe developed the expertise to transfer viral RNA from TMV into plant protoplasts.⁴⁹ One

⁴⁸ R. Malmberg, and R. J. Griesbach, "Chromosomes from Protoplasts - Isolation, Fractionation, and Uptake," pp. 195-202, In: Kosuge et al. (eds) Genetic Engineering of Plants: An Agricultural Perspective, 1983.

⁴⁹ For a more complete review of this field of protoplasts research see: H. Murakishi, M. S. Lesney and P. Carlson, "Protoplasts and Plant Viruses" pp. 1-56, In: Maramorosch, K. (ed) Advances in Cell Culture, Vol 3 (New York: Academic Press, Inc., 1984).

of the early pioneers in this area in the United States was Harry Murakishi at Michigan State University, a plant virologist. He moved into tissue culture and protoplast studies of plant viruses and provided an important research collaboration with Peter Carlson.⁵⁰ Murakishi took a sabbatical leave to work with Takebi in the 1970s and brought the techniques back to his land grant university home. Later, Takebe enjoyed a reciprocal stay at the Michigan State laboratory.

Murakishi's laboratory provided the doctoral home for Roger Beachy, who later became one of the foremost plant genetic engineering experts for his use of viral coat protein genes as a cross-protecting agent (i.e., internal "vaccine") for a number of plant viruses in various crop species.⁵¹ This became one of the first demonstrations of

⁵⁰ From the mid 1970s to the early 1980s, Murakishi and Carlson studied the phenomenon of virus cross-protection by taking advantage of "green islands" in virus infected plants and cell culture. When tobacco plants were systemically infected with Tobacco Mosaic Virus (TMV) normal green, virus-free sectors appeared against a background of yellowed-infected leaf tissue. When these green tissues were excised and regenerated into young plantlets, they retained a measure of resistance to subsequent inoculation. Similar results were obtained from callus cultures infected with TMV. (See pp. 38 and 41, In: Murakishi et al, Protoplasts and Plant Viruses, 1983). Such evidence for a diffusible agent in cross-protection would lead to the use of viral coat proteins for genetic engineering pioneered by Murakishi's student, Roger Beachy (See below). The diffusible agent was viral coat protein and/or its messenger RNA.

⁵¹ See: R. N. Beachy, "Producing Disease-Resistant Plants," p. 121-123, In: Research for Tomorrow, 1986. Also see: J. H. Fitchen and R. N. Beachy, "Genetically Engineered

agronomically important single gene transfers that could be of immediate use in the field and it was predicated and institutionally prepared for by these early labors in virology before "genetic engineering." This blend of plant virology and plant tissue culture was a critical technology in the developing infrastructure that would make genetic engineering possible.⁵²

The regeneration of protoplasts and the tissue culture technologies developed for raising plants from callus

Protection Against Viruses in Transgenic Plants." Annual Review of Microbiology 47:739-763, 1993.

⁵² One important aside must be made here -- much is made of the dichotomy between "biotechnology" and traditional research as a fairly recent source of stress in the agricultural research community. Institutionally, the stress can be seen much more as an evolutionary development, grounded in the "basic vs. applied," "laboratory vs. field" research schisms that have existed as part of the very nature of scientific agriculture from the earliest days. Plant virology, one of the founding disciplines that blended into agricultural biotechnology, was one of the key examples of this problem: it was an outlier in much of plant pathology as a whole, primarily due to the fact that viruses were not amenable to chemical treatments as were fungi at first, then bacteria later. Plant virology found itself torn between blind selection for resistance and a need to do fundamental research in hopes of finding a weak point in the virus life-cycle amenable to "magic bullets" or chemical fixes. Only control of insect vectors was amenable to the standard pesticide approach. Interest in such control helped create a unified subdiscipline of virologists who blended studies of insects, viruses and plants in a unified appreciation of the disease cycle. Maramorosch and Harris reviewed much of this work during this period in their 1981 textbook: Plant Diseases and Vectors, pp. 106-181. A similar problem existed for those interested in resistance to abiotic stresses such as temperature, drought, flooding or salt, and it is no coincidence that these researchers tended to be among the quickest to embrace and promulgate the so-called "new" biotechnologies.

through hormone manipulation provided the expertise that would be required for the eventual production of transgenic plants. In its own right, however, it first provided plant breeders with the hope of fixing the ideal genotypes discovered through standard mass selection and directed breeding experiments into permanent clonal lines. Tissue culture derived "seeds," either through embryogenesis or specially protected plantlets, were hoped to provide clonal fidelity (genotypic uniformity) to those crop species in which outcrossing was mandatory and where stably selfed, inbred lines with high genetic uniformity were not possible. Genetic gain, once captured, would not be lost through the inevitable recombination that occurs in producing seed from even the most desirable of parental types. This technology was thus, far from a threat to traditional breeding, one of the best potentials for its efficient use.⁵³

Initially, one of the most powerful appeals of the new tissue culture technology was due to otherwise unwanted

⁵³ At least theoretically. The economics of new technologies were always assumed to be favorable "down the road" based on assuming the inevitable progress that would be made through fine-tuned technological innovation and economies of scale. The fact that this was not always so -- and that this was fully apparent to practitioners outside the laboratory with their "real world" experience -- seems to be a given in nearly all such innovations. It was almost always a source of conflict and misunderstanding between "basic" and "applied" researchers.

It is equally true that neither side had a track record of being always right, thereby keeping the eternal argument about practicality and future direction -- such as seen above in the early comments by plant breeders on the promise of genetic engineering -- alive.

possibilities inherent in its mass production and selection capabilities. Called "somaclonal variation" it was seen as one more of the panaceas, especially desirable because of its strong ties to a traditional breeding mentality. In ironic fashion, it took advantage of the bane of clonal propagation -- that of reproductive infidelity of clones.⁵⁴ Rather than trying to eliminate such variation and mutation, as those interested in strict clonal propagation of desired genotypes were doing, these new test-tube "breeders" sought to enhance that variation, regenerate the variants and search for desirable mutations in agronomic traits in standard field trials, rescuing the desirable genotypes at the end.⁵⁵

Even more promising was the possibility of in vitro

⁵⁴ Regeneration of cells from culture had the problem that mutations occur in any cell division event and could compound in the regenerated plant. Additionally, in the mature plant, despite the standard ideal of totipotency, the actual genetic make-up of various tissues could differ. Some plants were indeed chimeras (a term used to designate genetically different sectors existing in the same plant or animal). Thus the source of the clone might create variation in the clone compared to the original plant's phenotype as a whole. For a contemporary discussion of this phenomenon, see: Y. Y. Gleba, 1977. Non-chromosomal Inheritance in Higher Plants as Studied by Somatic Cell Hybridization. p.775-788, In: Sharp, W. R., Larsen, P. O., Paddock, E. F., and Raghavan, V. Plant Cell and Tissue Culture Principles and Applications. Columbus: Ohio State University Press.

⁵⁵ This clonal infidelity was known as "somaclonal variation." Larkin, P. J. and Scowcroft, W. R. discuss its potential importance in "Somaclonal Variation and Crop Improvement," pp. 289-314., In: Kosuge et al., (eds) Genetic Engineering of Plants: An Agricultural Perspective, 1983.

selection. The idea was to subject cells or protoplasts in culture (before or during regeneration) to a stress -- whether a pathotoxin, salt, herbicide, or hormone -- and thus allowing only those resistant to the challenge to survive. Even tremendously low mutation rates were theoretically acceptable given the hundreds of millions of "individuals" capable of being screened in a single petri plate. The direct analogy to field trials, yet with the phenomenal efficiencies of scale, made it a powerful technology both scientifically and rhetorically.⁵⁶

Somaclonal variation was a logical outgrowth of earlier attempts to manipulate genetics by inducing mutations in a number of fashions, particularly common after the rise of the nuclear era, with the discovery of radiation at the turn of the century, and later after the atomic bomb. These mutation studies also attempted to develop selection techniques for a number of stress parameters. Their early successes not only provided genetic markers, but evidence of gene plasticity and the hope (if not possibility) of finding almost anything one wanted in existing cell lines or genotypes.⁵⁷

⁵⁶ Office of Technology Assessment, Genetic Technology: A New Frontier (Boulder: Westview Press, 1982) pp. 140-151., and R. S. Chaleff, "Isolation of Agronomically Useful Mutants from Plant Cell Cultures," Science 219: 676-682, 1983.

⁵⁷ N. F. Jensen, points out the early role of radiation in mutation analysis and the creation of genetic variation in Historical Perspectives on Plant Breeding

More important still, this technology provided the basis for the standard techniques ultimately used in plant genetic engineering. Desired gene transfer was accomplished in conjunction with the transfer resistance genes to particular antibiotics/toxins such that the selection scheme outlined above would allow one engineered out of millions of untransformed cells (in organized or unorganized tissues) to survive and be selectively grown into a transgenic plant.

Protoplast fusion and somaclonal variation both would later be derided as "revolutions" that failed their promise -- a rhetoric that was typically used by each new iteration of biotechnological development to describe past technologies and approaches.⁵⁸ They took a lesser place as one of a panoply of tools compared to the universal genetic engineering approach (direct DNA/gene transfer through the

Methodology, 1994. Years earlier, summarizing the state of the art at the time of agriculture's move into genetic engineering, B. Siggurbjornsson, discussed the role of "Induced Mutations," p. 153-176, In: The American Society of Agronomy (ed), Crop Breeding (Madison: ASA, 1980).

⁵⁸ According to Kung, S. and Wu, R. Transgenic Plants: Vol 1: Engineering and Utilization (New York: Academic Press Inc., 1993) pp. 6-7: "The dramatic success in producing somatic hybrids generated great excitement and great expectations in the 1970s. Hopes were high that this technology could permit unlimited generic exchange. . . . In most cases, not surprisingly, these attempts failed. . . . [However] . . . evidence from the era of hybrid plants to the era of somatic hybrid plants permitted expansion of the scope of plant breeding to both the organismic and the cellular levels." The next level would be "the molecular level" in "the new era of biotechnology or genetic engineering."

use of vectors) that they were originally envisioned as being. But without such precursor technology, neither the rhetorical space nor the institutional/scientist base could have existed for the rapid implementation of the quintessential biotechnology of recombinant DNA.

Failures and successes of these technologies were always judged in practical terms as opposed to scientific utility -- especially in the agricultural community. Although agriculturalists desired scientific credibility through the use of the newest techniques, they were committed to those that "worked" to practical effect and not "just" as research tools.

Plant and Animal Pathology

Next in order of historical success, but far more "showy" in results than the soil and nutritional analyses that formed the bulwark of agricultural physiological studies, were the developments in plant and animal pathology by USDA and land grant university researchers from the 1880s on. Similar to developments in plant and animal physiology, pathology research in the governmental research establishment focused on applied programs and was instrumental in forwarding the notion of agricultural chemistry into a broader range of "ecological" control. Pesticides and herbicides in the arena of plant pathology and pesticides and vaccines on the side of veterinary

medicine provided an added array of chemical "fixes" that could be applied to agricultural problems from the use of sulfur and Bordeaux mixture on grapes, to preventing cattle diseases with vaccination.⁵⁹

Further unification of the biochemical view of plants and animals was due to the development and study of nutritional diseases of both plants and animals. If the Loebian engineering model had any credibility in agriculture, it was because of these agricultural scientists who were cast in the role of "chemical engineers" and provided chemical fixes to diseases both infectious and nutritional.

On a more sophisticated level, the developing science of pathology provided agriculture with much of the educational/intellectual framework it needed to move into genetic engineering and the new biotechnologies. At the same time, it provided logical advocates for the goals and approaches the new techniques made possible. The massive financial and institutional commitment to "spray and pray" approaches that developed in plant pathology long before World War II, although finding its ultimate expression in the post-war chemical industry boom, provided one of the most powerful political and economic bases for the embrace

⁵⁹ USDA, A Century of Service, 1962, p. 32-33, details the conquest of cattle fever, the development of a vaccine and curative serum for hog cholera (p. 50), and dusting fields for boll weevils and pink bollworm (p. 132).

of the new technologies.

Many of the key players in the initial transformation of agricultural research into the biotechnology mode were trained pathologists or individuals with strong commitments to plant pathology, and, collectively, members of institutions (state, federal and industrial) where plant pathology and plant disease control were of overriding import and economic concern. Arthur Kelman provides an overview of the role of plant pathology's contributions to the biological sciences, which details some of the major technological and scientific developments.⁶⁰

Particularly important for the scientific background to modern biotechnology in agriculture were developments in plant virology, the genetics of host-parasite interaction, and the elucidation of the crown-gall-causing bacterium Agrobacterium tumefaciens' natural genetic engineering capabilities. Agrobacterium researchers helped provide a critical sense of the unity of life when they moved to obtain early funding from the NIH, arguing that the plant tumors caused were a model system for studying cancer research that would ultimately benefit human medicine. This not only helped to solidify the early role of NIH in plant research, but linked NIH to speculations on gene transfer,

⁶⁰ A. Kelman, "Contributions of Plant Pathology to the Biological Sciences." pp. 89-107, In: Frey, K. J. (ed) Historical Perspectives in Plant Science (Ames: Iowa University Press, 1994).

and tied the early indication of importance of hormones in plant diseases and routine plant physiology. This was an important connection unifying medicine and plants both conceptually and institutionally.

This early period was part of the pre-central dogma era. Starting in the early 1940s and '50s, the unifying concept for life, rather than DNA, was cell theory and the hope that all cells had certain commonalities of biochemistry and development that would prove useful to investigate. Plant pathologists in direct or peripheral ways were involved in much of the major progress with Agrobacterium, from Erwin Smith in 1907 to Braun in 1950 to the most modern breakthroughs by Mary Chilton. As an institution, the American Phytopathological Society (APS) would be one of the key groups moderating and validating the discussions of the Agrobacterium "controversy" going back to the initial days of proving its natural genetic engineering capabilities.⁶¹

For all of the insight regarding the key discoveries, strikingly lacking in Kelman's necessarily brief account are the institutional and individual ramifications of these and

⁶¹ This issue was still vociferously debated at the 71st Annual Meeting of the American Phytopathological Society held in Washington, D.C. (St. Paul: The American Phytopathological Society Press, 1979), two years after the original Chilton publication. (M. D. Chilton, M. H. Drummund, D. J. Merlo, D. Sciaky, and A. L. Montoya, "Stable Incorporation of Plasmid DNA into Higher Plant Cells: The Molecular Basis of Crown Gall Tumorigenesis. Cell 11:263-271, 1977.

related discoveries of the development of plant molecular biology as a whole. Certainly there is little consideration for their effects on the transformation of agricultural research. Although, he is particularly quick to note that, as predicted by Peter Carlson, one of the pioneers of advanced plant tissue culture techniques, plant pathology provided one of the earliest practical ramifications of plant genetic engineering -- the engineering of virus resistance into transgenic crop plant species by researchers such as Roger Beachy.⁶²

The 1990 successes reported are the results of research programs and industrial and academic institutional change that Kelman does not acknowledge. As Carlson and the land grant university pathologist, later research company entrepreneur, Albert Ellingboe, pointed out early on, the genetic "simplicity" of well studied plant pathogen systems provided the best bet for early genetic manipulation technologies to create agronomically important improvements.⁶³ Too many traditional yield components

⁶² Beachy discussed Producing Disease Resistant Plants in the 1986 USDA Yearbook, Research for Tomorrow, pp. 121-123.

⁶³ According to Ellingboe In Kosuge et al, (eds) Genetic Engineering of Plants: An Agricultural Perspective, 1983., p. 417: " The one fortunate characteristic of disease resistance is that it is possible to work with one gene at a time. . . . The commercially and biologically important problem of disease resistance can be state in a manner that makes these genes amenable to analysis by molecular geneticists. Disease resistance, therefore will probably be one of the first economically important traits to be studied

promised to be multi-geneic, complex systems too poorly understood and too genetically diffuse for biotechnology to tackle, especially in its early stages.

If anything, plant pathogen systems seemed deceptively simple, at first. The mindset of genetic determinism that plant pathology envisioned ever since Flor's development of the gene for gene hypothesis proved tremendously important to the goals and directions of agricultural biotechnology.

In the 1940s and 1950s, H. H. Flor, a plant pathologist, demonstrated that in flax, the so-called "rust" disease caused by the obligate parasitic fungus, Melampsora lini, a peculiar relationship existed between the genetic make-up of the host and its fungal parasite. Host plants and fungi could be separated, respectively into genetic varieties and races, each of which was compatible (entered into a disease association) or incompatible (no disease possible) with the other group based on single gene interactions. One gene for the host plant, one gene for the pathogen (fungus). In simple terms, host resistance gene A "recognized" fungal race gene A' (and no other) and was resistant. Host gene A could not recognize any other fungal gene and therefore any other fungal invader was not stopped and disease ensued. Flor and his students (such as Albert Ellingboe, later of Michigan State) would ultimately

by molecular genetics."

postulate this as a nearly universal pattern."⁶⁴

This research fundamentally changed plant pathology by providing a model of genetic reductionism for plant disease, and, more practically, a methodology for selecting for disease resistance based upon host varieties and identifying fungal races that would ultimately "solve" (without the slightest biotechnology input) the problem of rust in wheat production across the world. The notion of single gene resistance, so-called "R" genes provided a massively important research and rhetorical tool to genetic engineers who were later forced to argue their unique capabilities (and unique limitations) in being able to transfer single genes from one species to another. "R" genes provided the most cogent demonstration of the importance of single gene traits to agricultural yield. They also provided a built in model for countless research programs that embarked on quests to find and identify not only the genes, but the nature of their products, in order to attack the endless

⁶⁴ See: H. H. Flor, "Current Status of the Gene-for-Gene Concept." Annual Review of Phytopathology 9:271-296, 1971, and A. H. Ellingboe, "Genetical Aspects of Active Defense," pp. 179-192., In: Wood, R. K. S. (ed) Active Defense Mechanisms in Plants (London: Plenum, 1982). The tradition of support for gene-for-gene based molecular pathology continues to the 3rd generation with Dean Gabriel (Ellingboe's doctoral student at Michigan State University who would later become part of the University of Florida biotechnology program): "Gene-for-Gene Recognition: The Ion Channel Defense Model," pp. 3-15, In: Palacios, R. and Verma, D. P. S. Molecular Genetics of Plant-Microbe Interactions (St. Paul: APS Press, 1988).

problem of plant disease with the new technologies.⁶⁵ In addition, Flor's work did much to enhance coevolutionary concepts and to tie together biological unity of host and parasite in an intimate genetic dance crossing kingdoms and based on the gene (which was simply, as later shown, chemically embodied in the universal DNA).

Plant virology and animal virology sealed the question by providing proof that the physical genes themselves interacted across kingdoms, even across the biochemical boundaries between "life" and crystals. Wendell Stanley's crystallization of the plant virus, TMV, in 1935 provided a bridge between chemistry and life undreamed of in earlier generations.⁶⁶ It was a plant virologist, Myron Brakke, who, in the 1950s developed density gradient centrifugation and became a standard procedure for virology and macromolecular analysis for decades after.⁶⁷

The genetic mapping of Tobacco Mosaic Virus and others documented virus gene structure and function, and gave proof that foreign genetic material could not only producing

⁶⁵ Another example of "faith" in the entire research enterprise was the fact that, as far as "R" genes went, up through the 1990s, not a single such gene had been isolated and identified, even in a model system, much less in an agronomically important species.

⁶⁶ For an in-depth discussion of Stanley's work and influence see: H. F. Judson, H. F. The Eighth Day of Creation, 1979. pp.475-476.

⁶⁷ M. K. Brakke, "Density Gradient Centrifugation: A New Separation Technique." Journal of American Chemical Society 73:1847-1848, 1951.

foreign gene products in host eukaryotic cells, but were able to dominate host metabolism. Plant virology thus provided model systems for the very claims genetic engineering would require. This was the same sort of evidence that already existed for bacteria and bacterial viruses (phages) but was far more believable in practical, agricultural terms than one invisible organism being invaded by another invisible particle. It also demonstrated that across the biological world the same phenomenon related to DNA and the central dogma were indeed universal (and potentially manipulable to profit). No wonder with the demonstration that bacteria could be genetically engineered there was little doubt, at least to those with a plant pathology/plant virus bent, that the same would be doable for agricultural crops. In addition, a growing number of viruses that caused plant diseases showed themselves capable of cross kingdom infection, infecting and reproducing in the same fashion in both plants and animals.⁶⁸ Thus, philosophically, plant virology provided some of the most cogent arguments for the unity of life and the universality of the genetic material long before the existence of recombinant DNA.

⁶⁸ Insects - leafhoppers and aphids -- and nematodes, small worm-like organisms that live primarily in the soil, proved to be vectors of plant viral diseases at the same time as existing as hosts for these same viruses themselves. (See Maramorosch and Harris, Plant Diseases and Vectors, 1981. pp. 106-181).

Such work, and the discovery of DNA-containing plant viruses⁶⁹ made plant viruses an obvious early competitor with Agrobacterium for use as a genetic engineering vector. It would be several years before this possible utility was discounted on practical grounds that size of gene transfer would be limited, and, more importantly, stable integration and hereditary transmission of the transferred gene would not occur.⁷⁰

The quest for genes for disease resistance was based on a conviction of their existence and utilizability. The fundamental sense of unity between genomes of host and pathogen collaborated in disease and interacted in a concerted fashion at the molecular level. They provided a sense of unity across not only kingdom but even across the barriers of chemical "life" vs. cellular life. It provided a mix and match genetic mentality that agricultural scientists and administrators such as Al Wood would embrace as paradigmatic of the possibilities inherent in the new

⁶⁹ Most plant viruses contain their genetic code as RNA, which acts almost solely as the intermediary message in eukaryotic cells.

⁷⁰ In an almost amusing attempt to rescue a failing approach, there were those who proposed that plant viruses would prove optimal because they were not stably integrated. (See R. C. Gardner, "Plant Viral Vectors: CaMV as an Experimental Tool," pp. 121-142, In: Genetic Engineering of Plants (New York: Plenum Press, 1983). The claim was that plants could be infected with genetically engineered viruses at key points in their lifetime (say immediately before harvest) to produce desirable products -- (i.e. a high-lysine protein in corn). The insensitivity to public opinion of this approach was, to say the least, monumental.

biotechnologies. In a very real sense, it led to a mentality that treated organisms not as conglomerate bags of enzymes, but as aggregations of genes. Genes could be manipulated, modified and exchanged in tinker toy fashion to the delight of the engineering mentality. Resistance genes, avirulence genes and their associated mechanisms and biochemical pathways provided a chemical view of life increasingly more powerful for its promise of intervention in deleterious agricultural processes.

Plant pathologists such as Ellingboe would form a core group in many of the start-up plant and agricultural biotechnology industries of the period, and would, in federal and state agricultural institutions, lead the way in the shift not only to the new technologies, but to the genetic reductionism they entailed.

It is an interesting irony that Integrated Pest Management (IPM), one of the other key developments of plant pathology in the 1970s was a mainstay in reducing environment as well as host and parasite to mathematically modellable entities. Host/parasite genetics was a core component of this modelling approach -- the ultimate in engineering reductionism. IPM took the lessons from ecology and ecosystem sciences, coupled to the latest computer technology in an early effort to "solve" the problems created by industrialized agriculture. It was at heart the application of time management studies to the biological

community that was the farm. As industrialists embraced Taylorization for its economic and efficiency gains, so too did industrial farming look to IPM. It was an illusion for the organic farming lobby to see IPM as an ally, since its fundamental aims were not, as with Taylor's studies, aimed at helping the workers, but rather toward making the assembly line more efficient by eliminating threats to worker productivity. Still, IPM, because of its ability to predict and limit pesticide application, was seen as a friendly, "holistic" technology, often arrayed on the opposite side of the fight against what was seen by many as the molecular determinism of the new genetics and biotechnology.⁷¹

The truth of the underlying alliance of IPM with the mechanistic/reductionist vision of agricultural science is its co-option by biotechnology as the best means of implementing genetically engineered improvements. Engineering crops for herbicide resistance would enable the herbicides to be used at optimal points in the weed cycle, not only at those times when the host-plant was not yet

⁷¹ For a discussion of the theory and practice of integrated pest management see: W. H. Sill, Plant Protection: An Integrated Interdisciplinary Approach (Ames: The Iowa State University Press, 1982), pp. 211-230. In the 1991 USDA Yearbook, Agriculture and the Environment, pp. 150-159, Henneberry et al. discuss "Integrated Pest Management, A Sustainable Technology." They explicitly refer to it as "an ecological approach to pest suppression" and as a series of methods that "cause minimal environmental damage and pose little or no risk to human health."

present or small enough to be avoided during sprays or application. Resistance to pests or disease could be used to optimize fertilization regimes which were often counter-productive without heavy applications of pesticides (most insects and many pathogens (disease-causing micro-organisms) preferring fast growing and heavily fertilized hosts.

In addition, the lessening of pesticide and herbicide applications did not change the fact that at heart, the majority of IPM studies were, in reality, modelling the optimal use of traditional chemical fixes, not their elimination or replacement. It was less spraying with less praying. The requisite for stringent monitoring and control, sometimes through advanced microelectronics was built into the system in an ultimate triumph of "scientification" and chemical manipulation. It was, at the macro-level, the embodiment of the bioinformatics mentality that so transformed the concept of the molecular behavior of the gene, of DNA and the information character of self-regulation and control implicit in the central dogma of DNA.

Part of the proof of this implicit relationship was the ultimate IPM strategy for protection of plants from the abiotically caused "disease" of frost injury. Genetic engineering was used to produce bacteria that could, in theory, outcolonize and protect plants against their ice-promoting natural cousins, thereby protecting against a microbially mediated frost-damage. It was a complex and

subtle idea of ecological manipulation -- and completely anathema to the aims and goals and ideals of the "holistic" paradigm. It is also a note added in proof that plant pathology played such a prominent role in the early development and debate concerning not only agricultural biotechnology, but genetic engineering issues as a whole. It is also an excellent demonstration of the early corporate ties that developed in this area. The venture capital company, Advanced Genetic Sciences was the key industrial player in this area and an early victim to the first biotechnology backlash suits by Jeremy Rifkin.⁷²

Thus, in hearings before Congress in 1984, when a strong push to fund "alternative" agricultures was underway in response to the problems outlined in previous chapters of pollution and cost, IPM was raised in alliance with organic farming against traditional (here chemical, intensive, industrial) agriculture. USDA and its supporters countered easily, demonstrating that they were already at the heart of IPM research and that shifts to the new biotechnologies would enhance its benefits and provide the desired

⁷² James Shepard, a renowned plant virologist, was the key scientist originally involved with establishing Advanced Genetic Sciences when it was located in Manhattan, Kansas. At one point, the University of Florida attempted to lure Shepard to be a major player in its initial moves into biotechnology, offering him an endowed professorship. He refused, citing his commitment to Kansas State University and to his new company. (Letter from J. F. Shepard to K. R. Tefertiller, dated June 30, 1981. Gainesville: University of Florida Archives).

alternatives without the adoption of "alternative" regimes of no-tillage and "organic" farming, all of which were philosophically "dangerous" to industrial agriculture and the agricultural "establishment" of which USDA was a key player.⁷³

Ultimately, both figuratively and literally, plant pathology provided the vehicles necessary for the introduction and promulgation of plant genetic engineering across the agricultural spectrum.

Plant and Animal Genetics/Breeding

It is easy to dismiss the cavalier expressions of debt to plant and animal breeders expressed by biotechnologists as that of Oscar-nominees thanking all the "little people" who helped them throughout their career. But these were also the people who trained many of the new biotechnologists and, in fact, some of the most vociferous defenders of the new faith were late-in-life converts from the old.⁷⁴ Institutionally the power-structure of traditional breeding

⁷³ N. Shaller, Testimony, p. 15-17, In: Setting agricultural Research Priorities and Proposed Means of Implementation: First Session, October 7, 1993. S. Hrg. 103-415. Subcommittee on Agricultural Research. (Washington, D. C.: U. S. Government Printing Office, 1994). Shaller saw sustainable agriculture techniques such as integrated pest management as a potential area of compromise between traditional and "alternative" agriculture.

⁷⁴ See Jensen, "Historical Perspectives on Plant Breeding Methodology, pp. 179-194, In Historical Perspectives in Plant Science, 1994.

was not only the only source of germplasm and subsequent testing, but it held the reigns of power in connection with client-grower groups and federations. Breeders operated much of the institutional power structures that held sway over formula funding directed toward specific commodities. As the research establishment for genetic modification of plants and animals, the breeders held the infrastructure and power critical to the success of the "new" biotechnologies. Scientific developments in traditional breeding in the post-WWII era prepared the way for a rapid shift in research emphasis at the same time as political survival dictated its necessity. These developments had the effect of convincing the majority of institutional research practitioners that at the very least, accommodations must be made for the new technologies. New technologies removed many barriers to wide crosses that had hampered traditional breeding studies in the past. Chromosomal manipulation, including the use of colchicine, provided the ability to study chromosomes more accurately but also to induce ploidy changes more often and more repeatably in plant systems.

The redundant genotypes of polyploid plants and the lack of deterministic growth and complex organ integration of plants compared to animals allowed greater study of the physiological and genetic effects of extensive chromosome changes. Additionally, in plants the ability to manipulate ploidy influenced the ability to make wide crosses between

otherwise infertile species/genera. Embryo rescue techniques derived from tissue culture permitted the study of otherwise aborted genotypes. Clonal propagation techniques developed for ever increasing numbers of species allowed for the first time complex statistical studies on genotype environment interactions which were hitherto impossible except for highly selfed crop species.

The development of inbred lines (products of the kind of massive "selfing" (self-fertilization over many generations) provided the basis for the genetic studies and the production of hybrids that helped transform corn into a model both of traditional breeding success on a practical level and the best "mapped" agronomic species with regard to genetic markers. This "gene mapping" was based on genetic reductionism of phenotypic "trait" to a chromosomal region. In numerous crop species it provided the logical foundation for the application of more stringent biochemical genetics at the DNA level and the best hope for genetic engineering in a scientifically defined manner. Questions and frustrations that tormented the traditional breeders were promised answers by the new technology which seemed to be, in this instance, a logical outgrowth and potent assistant to the old.

In fact, what can be thought of as an earlier "biotechnology revolution" in assistance to plant and animal breeding was strongly underway based upon the central dogma

of genes producing proteins. Plants were mapped according to isozyme patterns to provide inheritance pedigrees which seemed directly tied to the genes themselves, without the excess physiological or pleiotropic effects that the larger tissue and organ and whole plant traits possessed.

Analysis of isozymes, and the prediction that isozyme variation was selectable variation for function, not just form, provided the exact analytical techniques for ultimate gene analysis. The practical technique of gel electrophoresis and the statistical analysis methods that would ultimately be used for DNA bands instead of isozymes were perfected by the breeders. They felt themselves directly in touch with genes through their products using the somewhat simplistic, but useful "one gene/one enzyme" model then in vogue.⁷⁵

Not only did these protein "bands" provide a visible "signature" of the genes in question, well-before the direct manipulation of the DNA was possible, they provided a physiologically significant end-product through which a "reverse-genetics" approach later promulgated by genetic engineers would prosper. Although it was notoriously difficult to find a direct link between an agronomically important trait and a protein band, success in human

⁷⁵ Pasteur et al. Practical Isozyme Genetics (New York: John Wiley & Sons, 1988) provides a detailed study of theory and practice behind protein electrophoresis and its use in detailed genetic studies for both plants and animals.

medicine, along with demonstrations of the phenomenal difference isozymes could make, provided examples towards which agronomists could aim.⁷⁶

The exhortations of the pathologists regarding the relative genetic simplicity of plant and animal diseases gave additional hope that isozyme technology would yield results. The quest for "resistance" proteins was intense in the 1970s and early 80s, especially among such researchers as Albert Ellingboe at Michigan State University (a direct inheritor of Flor's "gene-for-gene" hypothesis in plant diseases), who would be one of the leading exponents in plant pathology of a biotechnology approach thereafter.⁷⁷

Perhaps this isozyme technology to attempt to aid selection in the hands of "traditional" plant breeders was the greatest single preparatory step to subsequent RFLP (Restriction Fragment Length Polymorphism) and gene mapping technology-- both in terms of technical ability of the

⁷⁶ As discussed in Pasteur et al. Practical Isozyme Genetics (New York: John Wiley & Sons, 1988) p. 159, Linus Pauling, one of the pioneers of molecular biology, helped to determine through such banding pattern differences that there were structural changes between sickle-cell hemoglobin v. its normal counterpart.

⁷⁷ Ellingboe served as chairman of the session on "Challenges to Crop Improvement" at the first Conference on Genetic Engineering in Plants, held at Davis, California August 15-19, 1982. His opening remarks emphasized plant pathology's potential role in genetic engineering and the concept that genes to prevent "loss" may be as significant or more so than positive genes to "increase" yield. p. 416. In: Kosuge et al. (eds) Genetic Engineering of Plants: An Agricultural Perspective, 1983.

institutions, but even more so in the ability of individual researchers to understand, sympathize with and eventually utilize linkages and variation as expressed in gel patterns, the new lingua franca of the DNA world. Thus potential converts to genetic engineering and biotechnology were long in place and waiting for new solutions in the land grant institutions and agricultural research establishment well before recombinant DNA was a viable possibility.

CHAPTER 5 INSTITUTIONAL CHANGE/INSTITUTIONAL WARFARE

Whether revolutionary or not, the science that developed from tissue culture to genetic engineering was incapable of initiating the changes it did without considerable shifts and pressures in the social, economic and political milieu of the agricultural research establishment. It is easy to refer to the public agricultural research establishment as if it were a monolithic block. But in reality it was a dynamically evolving system. It was an externally defined and yet self-defining system with its own institutions, constituencies, agendas (sometimes mutually contradictory) and external friends and enemies both public and private, individual and institutional. It is important at this point to outline the government, private and industrial organizations that became involved in the debates.

In the late 1970s the key players in agricultural biotechnology at the federal level were USDA and its research arm, the Agricultural Research Service. Intermediary between the USDA and its state allies was the USDA/Cooperative State Research Service (CSRS) which served as a liaison to the state level State Agricultural

Experiment Stations, the majority of which were deeply intercalated in the administrative structure of Land Grant Universities. The National Association of State Universities and Land Grant Colleges contained a Division of Agriculture, and major committees on experiment stations and extension. The NASULGC was considered a de-facto government group to be consulted on any issues of large-scale policy. The majority of agricultural research from governmental agencies was formula funding (non-competitive) based on congressional budgets. These funds mandated some forms of research, usually in broad strokes, but left most others to SAES and USDA discretion.¹

General science funding was coordinated by the policy arms of congress and the administration in conjunction with scientific advisory groups. These were both governmental (the National Science Foundation - NSF, the Office of Technology Assessment - OTA,² the National Institutes of Health - NIH) and private (the American Association for the Advancement of Science - AAAS, and various think-tanks such

¹ Several reviews exist of the agricultural research establishment at this time. See especially Hadwiger, The Politics of Agricultural Research, 1982. pp. 12-31, and Kerr, The Legacy, 1987. pp. 149-168.

² The OTA, at Congressional behest, would be responsible for many of the analyses of agriculture and the potential of biotechnology in this period. It was critical of USDA especially and what it saw as wasted programs that overlapped with SAES research (OTA, An Assessment of the United States Food and Agricultural Research Systems, 1981. p. 4).

as the prestigious Winrock and Brookings Institutes). Most funds distributed by such groups were based on a competitive, peer-review system, deemed more scientifically objective at the time.

Lobbying groups with interests in agricultural research ranged from conservative to radical general membership groups such as the American Farm Bureau on the one hand, and the American Agricultural Movement on the other. Most agricultural subdisciplines had their own lobbying arms as did commodity groups. More recently, consumer groups and narrower private industrial associations such as the Industrial Biotechnology Association (IBA) were added.³

The majority of these groups, except of course for the IBA, pre-dated the so-called biotechnology revolution and proved useful supporters or early opponents of the shift to a greater and greater public investment in biotechnology research and development. Of more significance toward agricultural research was the economic and legal framework in which the new technology took shape within these institutions. This new framework helped make biotechnology a promise of salvation, financial and face-saving salvation at least. As shown in an earlier chapter, the now battered land-grant agriculture system and a USDA hounded by critics

³ The Industrial Biotechnology Association worked intimately with the LGUs both to develop research priorities, but also to lobby for government funds for biotechnology and for regulatory statutes that were the least onerous to the developing technologies.

on every side struggled not only against scientific assault but against grass-roots public protest. In addition, by 1980, with the election of Ronald Reagan, a relatively unfriendly and dismissive presidential Administration joined the opposition.

By the late 1970s, the corporation, Genentech, had demonstrated the feasibility of taking an animal gene, interferon, and inserting it into a bacterium. The technical mechanisms of transformation for splicing and cloning DNA in a commercial setting were thus in place. Numerous writers have detailed the effects of this "miracle" on Wall Street and on the propagation of entrepreneurial biotechnology companies.⁴ The sight of this wondrous fiscal prosperity afforded by genetic engineering was the envy and wonder of the land grant university administrator as much as it was to the general public. In the research administrator's case arose the question: why not my scientists, why not my institution? There were several reasons why not, most of them involved in the effort to keep research which was being done at government expense, especially in agriculture, safe from narrow private appropriation.

Agricultural research had always been envisioned as a critical component of the common good. It was often designed to benefit industrialization, but not usually particular

⁴ Teitelman, Gene Dreams, 1989. pp. 2-12.

companies -- except on a limited basis.⁵ This was the case until the Reagan era when privatization, profit-making and service to business was itself seen as a common good and "selling" public research to individual industries became a noble enterprise.

Several things had to change and in rapid order, before progress in biotechnology could be translated into respectable and legal profit at the land grant university. Up until this period, The Plant Variety Protection Act of 1970,⁶ allowed USDA to issue certificates of protection for sexually reproduced plants and was the only means of guaranteeing an economic return for private producers of commercial seed. But this act did not protect clonal plants (a critical need as the new tissue culture technologies came on board) and was not as strong a protection as patent rights. It made no allowances for public institutions to make profits on the plants that they developed and released. Not until 1980 was the ability to claim patent life affirmed by the Supreme Court in *Diamond vs. Chakrabarty*.

⁵ This was most often exemplified by the kind of "spray and pray research" indulged in by plant pathologists. From the beginning, many fungal pathologists participated in the development of effective fungicidal sprays and helped test new industrial compounds, often being paid to do so by industrial contracts. Many of the "applied" research articles in journals such as Plant Disease Reporter dealt with the results of testing such sprays under scientifically controlled circumstances -- a benefit both to industry and to academic careers.

⁶ This is discussed in depth by Kloppenburg, First the Seed, 1988.

Teitelman (1989) discusses the ramifications of such legal changes on entrepreneurial developments on Wall Street and as inspiration for faculty involvement in new start-up companies. At the University of Florida, for example, earlier patents were applied for for biological control agents, both viral and fungal. On May 25, 1977, R. Charudattan of the University of Florida requested information from the Chairman of the Patent Committee at UF on the feasibility of patenting an Argentinean virus as a biological control agent for milkweed. A patent application had already been submitted for the fungus Cercospora rodmanii, a pathogen of waterhyacinth by this same research group. ⁷

Equally, if not more importantly, Congress finally allowed land-grant universities to gain financially from patents on discoveries made within their jurisdiction.⁸ For public universities, then, the promise of a profitable interaction with industry was born, exactly at the time, and because, federal research support for non-military research was being withdrawn due to budgetary constraints.

Even though these dreams were crucial to infecting traditional land grant institutions with biotech gold fever, an initial source of start-up capital was needed to complete

⁷ Letter from R. Charudattan to M. Smutz. Subject: Patenting the Araujia Mosaic Virus. (Gainesville: University of Florida Archives.

⁸ Wittwer, pp. 315-317 In Busch and Lacy, 1986.

the picture.⁹ USDA competitive grants had only been initiated in 1977, and they were neither large enough nor focussed enough on the new technologies to provide the proper impetus. In the midst of grand schemes on Wall Street and pressures on its own biomedical organization to help push such research along, the National Institutes of Health (NIH) decided to give plant biology an opportunity to obtain biotechnology funding just as they had already provided for animal research.

This money was released before fears of NIH hegemony were becoming pervasive among the agricultural community. It was, in part, responsible for some of those fears later -- funding power in science always being one of the most powerful controllers of what research would or would not get done. At this point it was simply seen as a potential windfall for any university that could claim it, so the competition was entered with full fervor by a number of land grant institutions whose biotechnology programs were already underway.

The response to this at the University of Florida, under the research administration of Francis Aloysius Wood in IFAS, was not merely an example of a nationwide trend,

⁹ The start-up costs for biotechnology research were comparatively high, not only for equipping laboratories, but for hiring and training qualified personnel. Many of the land grant institutions were unable to redirect their limited discretionary funds in this area initially, and so depended on new grants as seed money for attracting further resources -- and further grants.

but also provides a background to the further development of biotechnology throughout the land-grant system. In July of 1980, Al Wood received a forwarded letter from D. S. Fredrickson, Director of NIH to Don Fuqua, a Representative from Florida and Chairman of the House Committee on Science and Technology.¹⁰ This letter is worthy of extended quotation for several reasons. It outlined NIH's commitment to finance new programs in plant biotechnology, but more importantly, is one of the best exemplars of the institutional warfare inherent in NIH's attitudes toward USDA. In addition, it is a critically important listing of those things the National Institutes of Health deemed worthy of funding in the modern plant biotechnology it was hoping to nurture.

It begins, after salutations, with a statement of intent about the need for more plant research to be done now:

Recent advances in recombinant DNA technology, coupled with increased understanding of biochemistry in plants, can and should be combined in an accelerated effort to study ways to increase both the quantity and quality of food. For humanitarian and economic reasons it is clearly in the best interests of the United

¹⁰ Letter to Don Fuqua, Chairman, Committee on Science and Technology from D. S. Fredrickson. Subject: Elaboration of NIH programs on molecular biology of plants, (Gainesville: University of Florida Archives). Apparently this letter was forwarded to Wood at UF via Fuqua. Fuqua received his B.S.A. from the University of Florida in 1957 and was past president of the Future Farmers of America for the state. Apparently Fuqua continued to maintain a friendship with the University of Florida and Florida agriculture.

States, as the largest exporter of agricultural materials in the world, to take the lead in this manner.¹¹

The following four research areas are suggested to be "strongly supported": 1) increasing the nutritional value of seed storage proteins by "introducing genes for nutritionally balanced storage proteins without reducing yield"; and 2) "It is now feasible to introduce genes for improved proteins, or, perhaps more importantly, introduce genes for resistance to a variety of pests into plants that are known to be agronomically suitable."¹²

As a further commentary the NIH Director relates:

Needless to say, success in the area of pest resistance could eliminate many problems of pesticide-associated pollution [sic]." He estimates that it would

¹¹ Letter to Don Fuqua, Chairman House Committee on Science and Technology from D. S. Fredrickson, Director of NIH, July 2, 1980.

¹² Letter from Fredrickson to Fuqua, 1980. It is interesting to note that this optimistic statement of current feasibility was made fully two years before the first concrete demonstration of successful transfer of eukaryotic genes in a plant system. This was something of which the land-grant community -- lambasted in the section that follows -- would have been well aware. Not to mention that even by the 1990s putative disease resistance "R" genes were yet to be isolated. It was one also one year before the announcement of the first successful gene transfer in mammals. As Rifkin reports in Algeny, p. 10, in September 1981, Dr. T. Wagner reported transferring a hemoglobin gene from rabbits into the fertilized egg of a mouse. The mouse contained and produced the "rabbit" hemoglobin as well as its own, and this was stably inherited. Early knowledge of this research may well have been a source of Fredrickson's optimism, but that it would prove of immediate benefit to plants, and that there were plant genes of agronomic interest that could be isolated and transmitted was completely speculative.

"take approximately two to three years from the time of the isolation of an altered, improved plant before the seeds would be available for commercial use. Therefore, the need for intense effort is at hand."¹³

With the rest of the plant research funding community, Fredrickson lists the typical additional areas deemed worthy as: 3) research on nitrogen fixation, including "intensified efforts via recombinant DNA technology, to introduce genes that "fix" nitrogen into plants that normally do not have this capacity. . . ." and 4) "Finally studies of chloroplasts . . ." not only for research into food production but because ". . . this organelle provides a special opportunity for the molecular biologist to derive basic principles of regulation that can be applied to animal cells and thence to human diseases."¹⁴

With nearly all of the above optimism and research goals, Al Wood would have certainly agreed. He was to make similar statements in the near future. He was likely puzzled by references to the seeming immediate feasibility of genetic engineering to the point of obtaining transgenic plants in any crop species -- but would show his own conviction later that it was indeed, only a matter of time.

¹³ Fredrickson to Fuqua, 1980.

¹⁴ Fredrickson to Fuqua, 1980. Here speaks the unity of life and the reductionist framework behind the interest in the biotechnology of plants from the mouth of the NIH director. Plant cells are to be studied because they can be seen as a useful model system for understanding animal cells and human diseases.

However, it is the latter section of the letter that was most threatening and disheartening. It was exactly the kind of message that had been percolating to USDA and the land-grant system over the years that had created a sense of crisis (as well as paranoia). As such, it too is worthy of extended quotation because it delineates the ultimate competition for pre-eminence that would ensue between NIH, NSF and USDA over biotechnology funding:

Funding of agronomical research has been primarily supplied by the USDA via grants to experiment stations and to land-grant colleges and universities, a system that has been successful and appropriate for investigation of local agronomical problems. It is difficult, however, to take advantage of new opportunities in basic research by such mechanisms. In 1977, the National Academy of Sciences prepared a report entitled: "World Food and Nutrition Study: The Potential Contributions of Research," in which it . . . recommended . . . increased funding in several areas of botany, including classical and molecular genetics, and photosynthesis. In response . . . the Office of Science Technology Policy, while recognizing that the National Science Foundation and the National Institutes of Health were intensively funding such research, suggested that USDA increase their funding of such projects by a competitive research grants program. Although the USDA has developed [such a program] the level of funding for plant molecular biology in 1980 (approximately \$1.2 million) was adequate to fund only 25% of the applicants at a dollar level equivalent to approximately 25% of the amounts requested. . . . More is needed. I believe that in order to attract well trained molecular biologists into plant research, a commitment to substantial, stable funding must be made by granting agencies.¹⁵

Fredrickson goes on to make the case that "strong peer review is healthy for such an exciting and important area of

¹⁵ Fredrickson to Fuqua, 1980.

research." Fredrickson closes his letter by stating: "In fact, I believe this to be so important that we at NIH will attempt to increase our support in this area in 1981. We would like to double our support of plant research in the form of competitive research grants."¹⁶ These would be administered by NIH's National Institute of General Medical Sciences.

In other words, NIH was moving into plant biotechnology funding at a level nearly five times greater than that of USDA and was planning to "expand research in this area within the intramural program of the NIH." The message sent to Fuqua, and conveyed then to Wood would certainly have been clear: USDA was incapable of doing the job properly, and NIH, already achieving status as the key government regulatory body for recombinant DNA research, was moving in to take up the slack in funding as well. Such a focus on the need for more competitive grants (but through USDA) would surface later in the NASULGC Committee on Biotechnology which would press for increased biotechnology grants.

The land grant university system was not yet ready to

¹⁶ Fredrickson to Fuqua, 1980. It is important to realize that lack of peer review was one of the chief criticisms raised against USDA research funding previously. Its accusers claimed typically that USDA and the SAES's formula funding acted as a sinecure for agricultural hacks and allowed continued pork barrel research to be done to satisfy commodity groups and politicians rather than the goals of professional science. This was an unfair criticism, if indeed it was intended to be launched against the new USDA competitive grants program (which was peer reviewed, despite being money-poor).

advance its own biotechnology funding, so the announcement of increased NIH funds in the area could not be ignored by any administrator seeking to develop his university's program. Previously Wood had appointed a recombinant DNA advisory committee at UF, an ad-hoc group of scientists that had been meeting for two years, and, in a sense, preparing for just such a funding opportunity.¹⁷ This pre-existing committee at the University of Florida, as with similar ones at other land grant universities indicated an early receptiveness to biotechnology even more surprising in the light of the limited practical possibilities at this time, especially for institutions supposedly whose research systems were only "appropriate for investigation of local agronomic problems." Wood called F. Bergman, Program Director, to discuss NIH grants and afterwards, informed the rDNA committee of NIH plans and advised it on August 8, 1980, to prepare to submit a grant for a Program Project.¹⁸ (Presumably on the theory that, if one could not beat them,

¹⁷ In March 1978, John Fulkerson was one of the first encouraging the University of Florida and Wood to move into rDNA research as shown by a letter from Vernon Perry to F. A. Wood. Fulkerson let Perry know that there would be Federal funds available and "less restrictive guidelines are also being planned." He sent information and a copy of HR-11192, Recombinant DNA Act dated February 28, 1978. The call to identify expertise in R-DNA lead to the development of a university-wide R-DNA committee which would lead the way in pursuit of biotechnology funding.

¹⁸ There is a memorandum from Wood (August, 1980) detailing his phone call to F. Bergman, Program Director, to discuss NIH grants. It distributed to the R-DNA committee with a suggestion that they form such a grant.

take their money).

The grant was hastily assembled. The committee met to put the finishing touches on the proposal on Monday night, September 29, 1980. Curt Hannah, chair of the Recombinant DNA Committee and "the girls" (several secretaries) stayed after hours to put the product in shape, one of the secretaries staying on until 5:15 the next morning.¹⁹ The total money request was for over 2.2 million dollars (more than USDA's complete competitive grant budget for 1980) and would have funded eight laboratories for five years. The basic focus of the proposal, although annoyingly diverse from at least the perspective of NIH reviewers, was on gene expression and transformation of corn.

Al Wood did active lobbying for the proposal, sending a copy directly to Fred Bergmann, Program Director of Basic Plant Genetics for NIH with whom he had conversed about the grant prior to development. In a letter dated September 30, 1980, he offered "to stop by your office for a few minutes during that period if you are available," when he was attending a national conference on Recombinant DNA Policy being held in Bethesda, October 9-10.²⁰ In addition, Wood sent a copy of the program project to Dr. Harold Hanson,

¹⁹ Letter from Hannah to Wood (October 10, 1980).

²⁰ The details and the availability of the conference proceedings are noted in a letter from J. Grupenhoff to Al Wood dated January 21, 1981. It was entitled: "Recombinant DNA and the Federal Government." (Gainesville: University of Florida Archives).

Director of the Science and Technology Committee of the House, asking for any "suggestions and comments after you have had a chance to review it."²¹ Hanson sent back a short note of approval with no details.²²

These efforts were to no avail. The grant failed for several reasons according to the NIH grant summary dated April 3, 1981, sent by W. C. Wardell, Privacy Act Coordinator to William Gurley, Agrobacterium researcher and coordinator of the original grant and acting chair of the Recombinant DNA Committee in Curt Hannah's absence.²³ A copy of the report was forwarded to Al Wood and Curt Hannah. The Florida effort received mixed reviews on the science, (five out of eight proposed projects were deemed "scientifically meritorious and were recommended for approval, although the level of enthusiasm varied among the Projects,"²⁴ but most

²¹ Letter from Wood to Hanson, dated October 7, 1980. (Gainesville: University of Florida Archives).

²² Letter from Hanson to Wood dated October 16, 1980. (Gainesville: University of Florida Archives).

²³ Letter from W. C. Wardell to W. B. Gurley, (April 3, 1981) enclosing Summary Statement re: the UF NIH grant. (Gainesville: University of Florida Archives). Hannah was on a development sabbatical at the time to "become familiar with new techniques in nucleic acid biochemistry in preparation for assuming the leadership in IFAS of the plant genetic engineering group. (Letter from Maynard to Tefertiller requesting faculty development leave for Curt Hannah, dated October 10, 1980 (Gainesville: University of Florida Archives)).

²⁴ Review sent to W. Gurley of NIH grant summary by W. C. Wardell, Privacy Act Coordinator, NIH. April 3, 1981. (Gainesville: University of Florida Archives).

especially mixed reviews on the nature of coordination and the administrative structure.

A Program Project was intended to draw together disparate research interests into a concerted effort on specific problems under one administrative umbrella. Additionally, it was intended to use common core facilities to supplement all researchers with equipment and technical expertise -- a truly collaborative affair. It can be argued that the failure of this grant, especially coming as criticism from the key biotechnology broker and main competitor with USDA for control, proved a powerful learning experience for all concerned.

Al Wood, as research administrator, quickly absorbed what gaining significant funding success in biotechnology would require. First, an impressiveness of scale -- the scope and importance and integration across disciplines of the actual research project proposed; second -- extensive administrative intra-institutional cooperation, including the development of common core facilities. And third, given the natural tendency to assume bias in criticism (perhaps not an unexpected or unjustified belief given the attitude of NIH to land-grant research institutions shown in Fredrickson's letter) -- it would seem to require not relying on NIH but working also for a return to more traditional funding bases where land-grant colleges had a competitive edge, or at least an unbiased chance, compared

to the assumed ivy-league prejudices against the agricultural establishment existing at NIH.

Al Wood's copy of the report has several sections underlined and checked in pen. Almost all of these refer to the lack of a proper administrative structure. One key section marked reads: ". . . the foundation for a Program Project is present at this institution and . . . with proper attention given to the organizational structure of a potential grant, a Program Project grant at this location is a viable future possibility."²⁵ And another, this one completely underlined: "The application would have been strengthened by the proposal of core equipment or other core funds to be shared among the participants."²⁶

One major critique might have made anyone familiar with Fredrickson's earlier letter cringe was a complaint that: "The proposals were almost uniform in their superficiality, the frequent extravagant claims which were not based on the availability of hard data, and the immense, but unfounded, optimism of the investigators about where these studies would lead."²⁷

²⁵ NIH Summary Statement for "Studies on Gene Structure and Function in Higher Plants" in Al Wood's archives. Attached to copy of letter to W. Gurley from W. C. Wardell dated April 3, 1981, p.1. (Gainesville: University of Florida Archives).

²⁶ NIH Summary Statement, April 1981, p. 4.

²⁷ NIH Summary Statement, April 1981. p. 3. Such a critique would have made an excellent summary of the times as a whole for biotechnology, in academia, government and

It is perhaps no coincidence then, that during and after this period, Al Wood was involved in a major push in biotechnology public relations among agriculture's traditional constituency, giving newspaper interviews²⁸, speaking at the Kiwanis Club²⁹ and writing articles for agricultural trade journals.³⁰ In all of this promulgation of the new technology, he used the rhetoric of the unity of life to point out that what was possible for bacteria was equally possible and desirable for crop species.

On a political level, Wood increased his developing ties with John Fulkerson, Principal Scientist at USDA/CSRS. Both men realized the need to prod the agricultural establishment itself into forward motion. This was typical of the institutional moves that agriculture had always made; it looked to its own, just as it had done when first faced with the new environmentalism. It had proved both a source of strength and weakness in the past.

Although the two men were brought together under administrative circumstances, Wood and Fulkerson would have had their interests in biotechnology (and their personal perspectives) tempered by their long term contact with The

Wall Street. Not to mention the director of NIH!.

²⁸ Hartley, 1980. p. B-3.

²⁹ Gainesville Kiwanis Club, January 14, 1981.

³⁰ Wood, IFAS Working for You, Florida Foliage, November 1981. pp. 17-20.

American Phytopathological Society. Their mutual background as plant pathologists proved a link beyond that of administrators tied merely by overlapping areas of influence. John Fulkerson had received his Ph.D. in plant pathology from North Carolina State; Al Wood his plant pathology doctorate from the University of Minnesota.

There is evidence that this plant pathology background strongly influenced Wood's vision of the scope of genetic engineering. He had long been aware of the intimate genetic relationships possible in plant diseases. Here was another vision of life's unity, even across kingdom lines as was shown in notes he made for a seminar on the natural origins of biotechnology wherein he speculated on the routine existence of "natural" trans-species genetic exchange. He was looking to other potential pathogens that might serve as vectors, and, trading on his forestry background, he considered the gall forming disease Fusiform Rust of pine as a potential for a fungal analog to Agrobacterium.³¹

Fulkerson, for his part, was a listed participant in one of the key plant pathology conferences in 1979 where the Agrobacterium story was debated in front of a large

³¹ Personal notes by Al Wood for a general biotechnology seminar, dated December 7, 1983. (Gainesville: University of Florida Archives). The idea of "natural" is a recurring and important theme that will be discussed further in Chapter 7.

audience.³² The non-coincidental association of Agrobacterium transformation systems (and the early hopes for using DNA plant viruses) might well have been significant. The traditional ties between plant pathology and the pesticide industry, which dictated some of the genes originally examined for transfer were surely important. This common background influenced the interest in biotechnology that both Wood and Fulkerson shared as former plant pathologists. It was one reason why each responded more enthusiastically to the need for change than many others in their position did, at least in the agricultural community. And, critically, it helped to explain their willingness, even enthusiasm, to establish industrial partnerships. This willingness was more familiar to plant pathologists than to many of the other agronomic disciplines.

In 1981, Al Wood, articulated his vision of the promise of agricultural biotechnology: "new and exotic hybrids of unrelated species, enhanced biological controls of various pests, increased efficiency of plant roots either by virtue of enhanced nitrogen fixation systems or mycorrhizal associations are just a few of the many potential benefits to be derived from contemporary genetic engineering research."³³ For three years (from almost the start of Al

³² Proceedings, 1979 American Phytopathological Society Meeting, Washington D.C.

³³ Wood, November 1981. p. 19.

Wood's tenure) "IFAS made a major commitment to increasing and broadening its research in these areas. . . ." ³⁴ such that by 1982 it was 3rd in the nation in total biotechnology dollar spending (\$3 million annually), lagging only behind California and Wisconsin. ³⁵

It is important to look at the promise of the new technology in terms of solving the agricultural crisis mentioned earlier. These promised benefits of biotechnology, as Al Wood listed them, are noteworthy in that they provide not only the promise of new plant varieties and increased productivity in themselves, but more specifically antidotes to the earlier purported "failures" of agricultural research. Instead of reliance on pesticides (the bane of "Silent Spring"), biotechnology offered biological control; in place of expensive fertilizers (disaster for the "Green Revolution") it offered greater root efficiency. Against the argument that agricultural scientist were merely applied research hacks was placed the picture of the most up-to-date agricultural genetic engineers. The use of unrelated species for gene sources promised to conquer the perceived yield barriers developing in traditional breeding approaches. Thus, the only promise lacking was that the tomatoes would not be hard.

³⁴ Wood, November 1981. p. 19.

³⁵ NASULGC, Emerging Biotechnologies in Agriculture: Issues and Policies, Progress Report XI, 1992. pp. 19-21.

None of this was taking place in a vacuum. The forces moving biotechnology forward at this time were immense. On Wall Street and in the laboratories of America, the new technology was a vital force. And of course the Federal Government was well aware of the developing "giant" under its nose. Everyone had seen the explosion of the computer field to the financial and technological benefits of Silicon Valley. It was in everyone's mind that biotechnology would do the same, and more, for the medical and agricultural world.³⁶ And this time everyone wanted to be in on the ground floor: financiers in investment, scientists in research credit (and research dollars), the Federal Government in funding and regulation (to achieve political capital, as much as any other investor might expect hard cash).

Years before, the USDA and the Land Grant University System had failed to prepare for, much less notice, the developing ecology movement fueled by "Silent Spring." They had similarly failed to prepare for the contempt of their peers for their lack of scientific rigor and advancement and the damning effects of their intensive technologies.³⁷ This time it seemed as if they were determined not to let this new force come into their lives outside of their control, or without maximizing the opportunities for potential profit

³⁶ Teitelman, Gene Dreams, 1989. pp. 2-12.

³⁷ Lear, Bombshell in Beltsville, 1992.

from the change.

For these reasons it was also on an institutional and political level that there was much to tie Al Wood and John Fulkerson together. Unlike the typical mandate of the USDA Agricultural Research Service (ARS), the primary research arm of USDA, the fairly recently initiated Cooperative State Research Service (CSRS), of which John Fulkerson was Principal Scientist, had a specific mandate to enhance the State Agricultural Experiment System and to improve State/Federal interactions. It was a classic states' rights/federalist dichotomy which would eventually be demonstrated in the different scientific agenda represented by spokesmen for the two organizations. Ultimately the SAES system was in direct opposition, by its behavior, to long-term ARS goals. In addition, their very existence was at times a threat to ARS laboratories who were often accused of needless duplication of SAES research in this period.³⁸

The moves that administrators such as Al Wood and John Fulkerson took to protect and promote land-grant agriculture were self-conscious, and political, to be sure. But all the while faith in the science behind the revolution appeared to sustain them in their forward push. This time they felt, as Wood continually proclaimed, that the technology was at hand

³⁸ OTA, An Assessment of the United States Food and Agricultural Research System, 1981. p. 4.

to change the world and presumably to reclaim American agricultural research from the scientific morass and political nightmare into which it had drifted.

Obviously, then, a key aspect of this period was the question of who would regulate and control the new technologies. It was critical to USDA to fight off dominance threats from NIH and NSF on the one hand (as scientific goal-setters and arbiter for biotechnology research agendas) and with NIH and EPA (as regulators and policing agencies) on the other. This battle was waged by "circling the wagons" of the state and federal agricultural agencies against outside "attack" and by a call for support from clientele and from individual research scientists and institutions who could be relied on to see and present the "special" situation of the agricultural community to sympathetic congressional committees.³⁹ In addition, moves were made to silence conflicting voices, either by the demeaning of traditional agricultural research⁴⁰ or by more conciliatory

³⁹ These were mostly from the "new" clients in industry such as William Brown (Pioneer Hi-Bred International Inc.), William Marshall (General Foods Corporation), John T. Marvel, (Monsanto Agricultural Products Co.) and Erwin Crosby, (National Food Processors Association), all of whom testified in support of increased agricultural research and advocated further movement into biotechnology. (Hearings before Committee on Agriculture, House of Representatives, 98th Congress, First Session, June, 1983. Serial No. 98-30), pp. 492-528.

⁴⁰ As exemplified by CSRS Principal Scientists such as John Fulkerson, who claimed that biotechnology would take corn from laboratory to field without breeder intervention. (Comment made to this author at a regional Forest Pathology

measures.

This conciliatory approach was exemplified by the rhetoric of Al Wood, Chair of the NASULGC biotechnology committee, who stated that biotechnology was not to replace, but to "serve" the older systems, all the while disingenuously urging the increased funding of biotechnology in an era of a recognizedly decreasing funding "pie." In almost every interview or paper he wrote, Wood consistently paid at least lip service to traditional agriculture (a wise course for a research director with so many "traditional" researchers beneath him). Wood also refused to equate biotechnology with genetic engineering, as many in this period were beginning to do. He continually stressed the importance of protoplast fusion and cell culture. The presence of Indra Vasil, his chief internationally famous biotechnology expert and key tissue culture researcher, may have had much to do with this routine inclusiveness. Vasil did no genetic engineering research per se.

By addressing the criticisms of a previous decade, and by grass roots lobbying and institutional changes fostered by the Land Grant University System, the majority of such challenges to USDA authority were fended off. USDA retained the bulk of control over its own implementation and regulation of the new technology -- despite tremendous

Meeting in Athens Georgia, 1987 in critical reference to my suggestion that breeders would always be needed to translate the fruits of biotechnology to the field).

pressures in other areas to conform to new mandates regulating both production research and multidisciplinary moves into social science realms.

The bulk of USDA and the Land Grant University System had failed to prepare for, much less notice until it was too late, the "Silent Spring" transformation of their world. Lost in mutual admiration with the conservative sector of their farmer clientele, they ignored market trends and social backlash. They neglected to shore up the rhetorical power of their production mentality which suddenly was now seriously called into question.

Flush with the governmental funding that flowed after WWII, especially at the university level, they had ignored what many now refer to as the process of entering a steady-state condition as opposed to continued growth. In the early 1980s, with biotechnology the potential "solution" to all of these problems, they were determined not to let this new and potentially saving force slip from their control. And institutionally there seemed to be considerable danger of that taking place.

In 1981, the Federal Government was determining who would rule and regulate the world of biotechnology. To the eyes of Congress, apparently, it was not so much an issue of the unity of life as the unity of administration. Government officials were determined to maintain authority with the National Institutes of Health. NIH had been the primary

governmental technology to deal with the issue of recombinant DNA even before the famous Asilomar conference in 1975, when the scientists involved (who still thought they were in control of their own research destinies) met prominently to discuss the issue.⁴¹ After that, it seemed logical to maintain NIH authority.

USDA and Land Grant Agriculture administrators had begun their big push into genetic engineering research. Despite earlier disinterest and claims that agriculture had been used to monitoring and controlling its own destiny, and despite their belief that USDA had a sufficient track record of regulating food safety issues in protection of the public trust, they realized that it was necessary and politic to transmit some regulatory stance to their members and associates. Thus, after several years of no firm policy, they essentially adopted the NIH guidelines in their entirety and sent word down university and research institution channels.

A Memorandum from Al Wood to Department Chairmen and Center Directors dated May 21, 1981 states: "We recently received a memorandum . . . describing procedures for the review and approval of all projects involving recombinant

⁴¹ James Watson and John Tooze in The DNA Story: A Documentary History of Gene Cloning, 1981., pp. 63-64. pointed out how in October of 1974, the director of NIH formed the Recombinant DNA Molecule Program Advisory Committee (RAC) to develop guidelines to regulate and control the safety of investigators and the public with regard to the technology.

DNA research funded either in part or in total by USDA fund sources. . . . An examination of the procedures reveals that they are similar to the current NIH guidelines which IFAS and the University of Florida are following." In fact, as the attached memorandum from USDA Recombinant DNA Research Director C. Grogan shows, the requirements for an institutional biosafety committee recommended that institutions receive copies of procedures from the Office of Recombinant DNA activities, NIH!⁴²

In one very real sense this was not a remarkable concession, because, with the medical/animal chauvinism inherent in the development of the original guidelines, genetically engineered plants were not a significant issue. Agrobacterium, the key potential vector being researched for plant genetic engineering, was not even mentioned, and thus was not categorized under regulatory control.

A far greater power struggle was occurring as NIH and other agencies were determined to carve out a major chunk of plant biotechnology for their own in terms of the most powerful controls -- governmental research funding and administration. From the very first groups such as NIH and NSF were contemptuous of the role of USDA in its handling of its own research affairs. Critical reports in the past by

⁴² A Memorandum from Al Wood to Department Chairmen and Center Directors dated May 21, 1981 containing an attached memorandum from USDA Recombinant DNA Research Director C. Grogan (Gainesville: University of Florida Archives).

"real" scientist reviewers have been cited above.⁴³ Both NIH and NSF had taken a leading role in funding fundamental research in plant biology, helping thus to define it, despite the fact that USDA had only recently begun its own competitive grant system in 1977, administered through the CSRS, partially in response to just such criticisms.⁴⁴

The National Science Foundation was in the business of funding plant biotechnology research from the start, and had, in fact, helped USDA set up its early competitive grants program in 1976. They were not seen as the threat that NIH was, in part because of the early close collaboration and in part because NSF routinely testified on behalf of expanding biotechnology and competitive grants funding in USDA before Congress. Additionally, when Congress refused to expand grant requests to the size USDA desired/required, NSF grants budgets tended to be increased to pick up that slack, insuring that at least some source of increased funding, still sympathetic to USDA would be available.⁴⁵ Still, it was not optimal to have this

⁴³ See Chapters 3 and 4 and Fredrickson's letter above.

⁴⁴ Ruttan, V. W. Agricultural Research Policy (Minneapolis: University of Minnesota Press, 1982) pp. 225-226.

⁴⁵ Mary Clutter, representative of NSF funding at the "Genetic Engineering of Plants Conference" pointed out in some apparent pride that "NSF's investments reflect your interests; that advances in science establish priorities for funding." NSF funding, she showed in a series of charts matched many of the USDA competitive grants interests, but at higher funding levels. It is pertinent that NIH

Congressional default position denying USDA its "rightful" share of funding and the SAES system was eager to push for more.

John Fulkerson, in his role as CSRS Principle Scientist, was well aware of this tension between institutions and the need to carve out agriculture's stand. He served as contact person on many of the joint government committees established to examine the issue of biotechnology in agriculture. Fulkerson sounded a warning to the land grant universities, especially to friendly Experiment Station Directors such as Al Wood: "I believe we have laid some very good groundwork here, but your Directors have got to move to keep the "high ground" -- stay out ahead of them."⁴⁶

Of course, the most dangerous "them" was NIH. It was feared, with good reason, that the biomedical establishment would steal all policy-making power and control of basic research funding in agricultural biotechnology from USDA. Given the relationship of mutual suspicion on both sides, NIH's usurpation of power would have been disastrous, at least from the traditional agricultural point of view. The basic research policies of organizations such as NIH, and

representatives were not present. Kosuge et al (eds) Genetic Engineering of Plants: An Agricultural Perspective (New York: Plenum Press, 1983), pp. 468-470.

⁴⁶ Letter from John Fulkerson to Wood, March 28, 1981. (Gainesville: University of Florida Archives).

even NSF, was fundamentally at odds with the applied research that agriculture's typical formula funding had espoused. And the contempt of their research administrators for "a system that has been successful and appropriate for . . . local agronomic problems" was well known.⁴⁷

Even though USDA had moved ever more into competitive research funding and had become more basic in many of its funding initiatives, its traditional concerns for commodity research had to remain a priority for political as well as scientific reasons involving a background of research knowledge and support. It was equally important, however, to maintain control of agricultural research as the special purview of USDA. This was true as a matter of policy, but also as an issue of training future researchers, locating and defining projects, and setting national priorities in an integrated fashion. One of the largest fears of the land grant universities at this time was caused by the sight of private and federal dollars going to agricultural research at private and for-profit universities.⁴⁸

Significant loss of research funding down the NIH or NSF pipelines would (and did) mean significant diversion of funds outside the established agricultural system.

⁴⁷ D. S. Fredrickson letter to D. Fuqua, 1980.

⁴⁸ Rule, Biotechnology: Big Money Comes to the University, 1988. details how private universities were capitalizing on biotechnology research and attracting new and massive funding both public and industrial.

Congressional testimony from USDA administrators had almost always resisted any attempt to weaken USDA's role in its own affairs, whether by establishing new research programs outside of its control or joint panels to evaluate the agricultural system. USDA officials had seen themselves hoisted on too many petards throughout the '60s and '70s. With the politicizing of agricultural markets occurring in the late 1970s⁴⁹ they were even less willing to let such a principal area of potential self-rescue slip from their in-house control.

Front-end lobbying and behind the scenes planning began in earnest. IFAS professors arranged and presented a session on genetic engineering to the Florida Farm Bureau.⁵⁰

By the end of April 1981, as Dean for Research, Wood had designated an advisory council of department chairmen and a faculty group with a chairman to project and

⁴⁹ President Carter established a grain embargo to "punish" the U.S.S.R. in 1979 for their invasion of Afghanistan. This disrupting American sales of wheat. Thereafter, especially entering the Reagan years, a series of massive trade negotiations (such as the General Agreement on Tariffs and Trade - GATT) in which agriculture, although involved was a junior partner to industrial concerns, played havoc with long term agricultural planning. (See Bonnen et al. Further Observations on the Changing Nature of National Agricultural Policy Decision Processes, 1946-1995, 1996. pp. 142-143).

⁵⁰ Letter from Wood to Walter Kautz, President Florida Farm Bureau Federation, dated April 23, 1981. (Gainesville: University of Florida Archives). It should be remembered that the Farm Bureau, nationally, was one of the staunchest supporters of the land-grant universities, and would remain a cheerleader in the area of genetic engineering.

coordinate IFAS-wide research in genetic engineering, designating it as a "priority among IFAS programs for the future."⁵¹

In May at the Southern Directors Spring Meeting, a panel discussion led by Wood and Fulkerson was held. Scientists such as Indra Vasil discussed protoplasts and plant genetic engineering, J.R. Wild discussed recombinant DNA, and Fulkerson and Wood discussed USDA and experiment station research policy and structure in genetic engineering respectively.⁵²

To keep on top of the national situation, Al Wood himself attended meetings such as the February 1981 conference in San Francisco, entitled First International Congress on Recombinant DNA Research. This meeting saw the government, industry thinktanks, and universities discuss the new technology.⁵³ In April, 1981 the Batelle Conference

⁵¹ Letter from C. Dean, Chairman of the Agronomy Department to the Crop Breeding and Genetics Program Area Committee, April 29, 1981. (Gainesville: University of Florida Archives).

⁵² Program, Panel Discussion on Genetic Engineering, Souther Directors Spring Meeting, May 4, 1981. (Gainesville: University of Florida Archives).

⁵³ As the summary by James Wild in Al Wood's files indicated: "The philosophy still seems to be that the technology will ultimately provide the means to as an infinite variety of questions about regulation of gene express, differentiation and development. The answers to these questions will allow us to understand and control development in a variety of systems and direct inter-specific gene expression." Key areas mentioned were: biological nitrogen fixation and host-pathogen interactions, among many others. (James Wild, Summary of First

on Genetic Engineering was held in Reston, Virginia. Here, as a summary of the conference report in Wood's files indicated, it was projected that NIH would relax its regulations of rDNA technology, at the same time as the EPA would "be monitoring environmental considerations."⁵⁴ Given the moves of NIH to dominate research funding, and the tremendous hostility of the agricultural establishment to EPA regulation as evinced in the earlier "Silent Spring" days⁵⁵ it could not have been a well-received revelation. Much more heartening, however, was the report of Tim Hall from the University of Wisconsin. Hall reported that the bean gene phaseolin was successfully transferred into Agrobacterium and integrated stably into a host plant. "It was predicted that within five years, it will be possible to transfer any gene into plants, have those genes maintained, and create novel genotypes."⁵⁶ The real problem, Wild reported, was to have those genes expressed properly. It would not be for two more years before transfer and successful expression was to be achieved, well after the

International Congress on Recombinant DNA Research, 1981. pp.2-4. in Al Wood's Archives. (Gainesville: University of Florida Archives).

⁵⁴ Wild, J. Summary of Batelle Conference on Genetic Engineering, 1981. p. 3. (Gainesville: University of Florida Archives).

⁵⁵ See discussion in Chapter 4 of this dissertation.

⁵⁶ Wild, J. Summary of Batelle Conference on Genetic Engineering, held April 6-10, 1981 in Reston Virginia. p. 2. Wood's copy.(Gainesville: University of Florida Archives).

conviction of eventual success had caused major shifts in research priorities and the embrace of biotechnology as the "research of tomorrow."

Despite such uniform optimism, institutional realities had to be confronted. Or perhaps because of the tremendous possibilities seen, it was even more necessary for USDA and the land-grant institutions to protect this future as their own. To that end, cooperation at state, federal and private industry levels of the agricultural establishment was intense.

Fulkerson, in his role as CSRS Principal Scientist, attended meetings with NSF, the Office of Technology Assessment, and the Department of State/OES among others. As he deemed necessary or prudent, he forwarded some of the in-process reports to various SAES directors, including Wood. As was expected, none of these reports was any more favorable toward USDA as a research science institution than previous ones had been. Some of these confidential government reports were leaked before completion and publication to expand and facilitate the warning, as if such were required after the likes of Fredrickson's 1980 letter. For example, one letter from Fulkerson to Wood, dated December 13, 1981 containing just such a jaundiced report concludes: "P.S. Enclosed is the draft of that report I promised you -- use care, please." At the top of the letter

Al Wood put: "CC. Wershow 1/5 (Confidential)."⁵⁷ Even at this point, the legal and institutional ramifications of expanding and regulating the new technology were still paramount, because so few decisions had been made or guidelines given.

Such warnings and exhortations would be a continuous pattern for the CSRS scientist. More than two years later, in the minutes of the March 1984 meeting of the NASULGC Division of Agriculture's Committee on Biotechnology, the summary of John Fulkerson's presentation related that "The current positions of the NIH, RAC, NAS, EPA, APHIS, ARS and other agencies were reviewed. In many instances, regulatory issues are political gains played to control publicly supported research vis-a-vis assessment."⁵⁸

Thus the Committee, and Fulkerson in particular, was ever mindful of the stakes when dealing with sister agencies. His access and connections to these other agencies made him an invaluable liaison, or even a "mole." Again he exhorted that "we must continually be ahead; to do otherwise

⁵⁷ Letter from Fulkerson to Wood, dated December 13, 1981. (Gainesville: University of Florida Archives). The Wershow referred to was James Wershow, the University of Florida agricultural law professor who would eventually serve as the key legal front-person on the NASULGC Biotechnology Committee.

⁵⁸ Committee on Biotechnology, Minutes, March 1984. p. 2. (Gainesville: University of Florida Archives).

will put agriculture in a negative position."⁵⁹

The southern directors, Wood key among them, especially heard the warning about losing their chance to maintain autonomy in biotechnology in 1981. They met to establish the first of the national research priority grants, with Al Wood as the titular head, to establish a position as being in the forefront of the new research -- before someone else beat them to the punch. Soon Al Wood and other bastions of the agricultural research committee devoted to biotechnology were testifying before Congress, in part with the help of friendly committeemen such as Don Fuqua. They set up the case for the needs of the agricultural community, for their ability to follow through if given the chance, and especially for the promise advanced by the new technology and the special role that plants held in the forward motion of the new science.⁶⁰

Well before the 1982 Winrock meeting that Martin Kenney cited as providing the key kick to LGU biotechnology efforts, the word had gotten out sufficiently to the land-

⁵⁹ Committee on Biotechnology, Minutes, March 1984. p. 2. (Gainesville: University of Florida Archives).

⁶⁰ For example, Bill Baumgardt, Director of the Indiana Agricultural Experiment Station, and Associate Dean of Agriculture, Purdue University testified "as a representative of the NASULGC Committee on Biotechnology" and provided a copy of the "Silver Bullet" to the committee members while advocating its policies for increased biotechnology funding. Testimony at Hearings Before the Committee on Agriculture, House of Representatives, June 1984. Serial No.98-70. Part 5, pp. 7-10.

grant institutions for a major collective push to be made.⁶¹ USDA/ARS was under the intense scrutiny of OTA and the Office of Science and Technology Policy and was preparing their response. In the meantime, SAES administrators realized it was time to make a strong assessment of their own and to take charge before someone else did. In April 1982, the Division of Agriculture of the National Association of State Universities and Land Grant Colleges (NASULGC), under the encouragement of its president, Kenneth Tefertiller, from the University of Florida, established its first Biotechnology Committee. The committee was chaired by Al Wood, Tefertiller's protege.⁶²

A key issue was that of insuring that USDA and the SAES retained the ultimate control of agricultural biotechnology. Meetings followed. Subcommittees were established on 1) Land-Grant Institutions (their infrastructure and agricultural and biotechnology research capability); 2) Funding and University Relationships; 3) Education and Manpower; 4) National Program Leadership and Development (including coordinating and evaluating the SAES

⁶¹ Kenney, Biotechnology: The University/Industrial Complex, 1986., pp. 232-233.

⁶² Tefertiller had hired Wood at the University of Florida in part to promote the move into more basic research and pushed the move into biotechnology both locally and nationally. At the time of taking up the role of chairman, Al Wood was comparatively unknown in the area of biotechnology, much less higher level NASULGC administration.

biotechnology programs); and 5) Social/Ethical Issues (related to application of the new techniques to agriculture). John Fulkerson attended most meetings of the new committee in his coordinating role as CSRS Principal Scientist, and was consistently thanked in all annual reports during his lifetime.⁶³

The first report, released in Fall 1982 was preliminary. Next came a prototype contract for university/industrial interactions. This sample contract was simply a section abstracted and bound from the original report. Prior to this document a sample contract from Agrigenetics (a private biotechnology company) was in circulation among the Southern directors and others. It was received by Al Wood in a letter from D. Smith, Associate Director of the Texas Agricultural Experiment Station, on October 2, 1981. Wood in turn forwarded it to K. Huston, Director-at-Large of the North Central Region of the SAES, on October 29.⁶⁴ It, or very similar ones, served in part as a model for the report contract. In all there was continued interest in privatizing biotechnology funding within traditional and new client groups, and the hope to take advantage of industry's raised interest in the new

⁶³ Committee on Biotechnology, NASULGC, Emerging Biotechnologies in Agriculture, Progress Reports I-VII, 1982-1988.

⁶⁴ Contract from Agrigenetics, enclosed in letter to Al Wood from D. Smith, October 2, 1981.

technologies and the new laws which allowed for large-scale financial collaboration.

There was one obvious revolution underway: private university companies and new cooperatives ventures with industry were exploiting the financial promise of the new technology. As newspaper items from across the country in Wood's collection show, issues of industrial and university cooperation were on everyone's mind. There were numerous university/industry deals cut during this period in the early '80s.⁶⁵

In fact, these developments in the private university sector helped drive initiatives being taken by the NASULGC Biotechnology Committee. There was the appalling specter of governmental agricultural research dollars shifting from the land grant universities to their private competitors to consider. There was also the money to be won from industry, which was threatened by the hunger of these same ivy-league competitors.

Still, industry was not yet envisioned as the perfect partner. Public universities often saw as much to fear for academic freedom as their private sisters.⁶⁶ In addition, there were fears of industry competition with the public system for qualified scientists. Wood's own papers contain

⁶⁵ Rule, Biotechnology: Big Money Comes to the University, 1988.

⁶⁶ Hart, K. "Is Academic Freedom Bad for Business," Bulletin of the Atomic Scientists. April, 1989., pp. 28-34.

expressed fears of such shortages, not to mention several requests from entrepreneurial groups and companies interested in coopting the efforts of university personnel for mutual research ventures.⁶⁷

Equally of interest was the desire to gain increased federal funding. In a significant blind-spot indicative of their ties to specific funding sources, the committee complained about an across the board decrease they perceived in federal research dollars. This was called into question by industry, which reminded Wood that NIH and NSF basic research funding was on a remarkable increase and only USDA was receiving short shrift. From the Committee's perspective, of course, this was the issue.⁶⁸

Other issues faced the adoption of the new technologies, not just these economic ones. On a broader societal level, there was fear of public backlash. Lawsuits by Jeremy Rifkin (the self-proclaimed prophet of genetic-

⁶⁷ For example, University Genetics (UGEN) was organized to provide research grants in genetic engineering as "a privately funded technology transfer company based in Connecticut." (A Memorandum from John Gerber to the biotechnology-related faculty at UF distributed UGEN's information packet. Dated: September 1, 1981. Gainesville: University of Florida Archives). Wood received a letter from Vencon Management Inc. which was "searching for various projects in the fields of biotechnology (genetic engineering) which could benefit from a venture capital investment." (Letter to F. A. Wood from Irvin Barash, President Vencon, dated July 14, 1981. Gainesville: University of Florida Archives).

⁶⁸ Letter to Al Wood from W. Marshall, General Foods Corp., dated February 24, 1983.

engineering "doom") fueled public fears as did numerous popular books promulgating the dangers of genetic engineering.⁶⁹ They also created consternation in the agricultural academic community which tended to feel that only scientists should judge any safety or social issues inherent in the new technology. Concern was forcefully expressed over public gadflies such as Rifkin and others, not to mention the unpredictability of state and federal court action. Most of Rifkin's arguments were direct counters to any concept of life's intrinsic "unity." His most cogent warnings relied on fears of tampering with the diversity of life and breaking down barriers that "nature"

⁶⁹ Jeremy Rifkin and his continuing anti-biotechnology suits continued to be a discussion topic throughout Wood's tenure and beyond in the Committee on Biotechnology. His tactics were an avowed reason for educational efforts and for advocacy of regulations that would assuage public fears. Rifkin's book, Algeny, which critiqued genetic engineering had tremendous public impact. The inside cover page uses provocative quotes from Senator Mark Hatfield: "This book may well be one of the most important documents of the decade." and Senator Al Gore: "This book raises questions that must be addressed as we achieve an ever-greater ability to control nature and as we move rapidly toward the ultimate technology: human genetic engineering." That such a man would be at the forefront of attempting to stop agricultural genetic engineering research created real problems for the agricultural research industry. But Rifkin was not alone. Others who discussed the ethics and dangers of the new technology for the popular audience included: J. G. Goodfield, Playing God: Genetic Engineering and the Manipulation of Life (New York: Random House, 1977), R. Hutton, Bio-Revolution: DNA and the Ethics of Man-Made Life (New York: New American Library, 1978), and N. Wade, The Ultimate Experiment (New York: Walker and Company, 1979).

had intended and imposed.⁷⁰

Facing such an onslaught, the agricultural research community embraced the idea of regulation, not from any perceived need for protection, but to assuage public fears. In addition, as further protection, government officials had to be wooed to the side of the traditional agricultural establishment for the development of the new technologies and the necessary regulations, not to mention for formulating new grant opportunities.

To these ends, in fall 1983 the second annual NASULGC biotechnology report, the so-called "Silver Bullet" was released. "Emerging Biotechnologies in Agriculture: Issues and Policies, Progress Report II," was the compilation of a select committee of deans and directors of various land grant universities and agricultural experiment stations across the nation (North Carolina, Indiana, Oklahoma, Idaho, Texas, Iowa, California, New York, Minnesota, Pennsylvania,

⁷⁰ Rifkin borrowed and extended Joshua Lederberg's "algeny" metaphor from the description of the process of transmuting biological entities (as "alchemy" would transmute lead into gold) to exemplify the philosophy of life that biotechnology made possible: "For the algenist, species boundaries are just convenient labels for identifying a familiar biological condition or relationship, but are in no way regarded as impenetrable walls separating plants and animals. The algenist contends that all living things are reducible to a base biological material, DNA, which can be extracted, manipulated, organized, recombined, and programmed into an infinite number of combinations by a series of elaborate laboratory procedures. . . . The algenist is the ultimate engineer." (Rifkin, *Algeny*, p. 17.). Rifkin, of course, opposed the new philosophy. Wood et al, embraced it.

Florida, and Wisconsin). It called into service a small number of professor-experts in biochemistry (North Carolina), economics (Iowa) and agricultural law (Florida).

This opening shot in the war for ag-biotech self-autonomy, unlike earlier preliminary reports, included survey data and well-established positions on the nature and needs of the new technology. It contained a large-scale proposal for "A National Program for Basic Research in Biotechnology for Agriculture and Food." It included prescriptions for the development of new institutions and new competitive grants programs specifically for biotechnology in agriculture. The entire document was a manifesto of what could be done, what was needed to be done, and the goals for which the Biotechnology Committee intended to lobby.

Each of the prevailing issues of biotechnology and privatization was carefully addressed. The document examined income tax credit structures for increasing research activities, opportunities for university income from patents and certificates for plant variety protection. There were further in-depth guidelines for the development of a university/industry research contract, including a sample contract. The thorny issue of faculty consulting for pay in the private sector was addressed and a review was given of the committee's interest in the current Lawsuit-Civil Action 83-2714 instituted by Jeremy Rifkin against the genetically

engineered "ice minus" plant bacterium developed by the University of California and Advanced Genetic Sciences.⁷¹

The goal of the document was clear in its attempts to point out the unquestioned promise of the new technology and the opportunities that existed. It only awaited government and private industry participation with the land grant universities. Minutes of subsequent committee meetings and letters to and from industry confirmed and extended these interests.⁷²

One of the committee members, Bill Baumgardt, Director of the Indiana Agricultural Experiment Station, testified on the "Silver Bullet" to Congress in June 1984, lobbying strenuously for an increase in the USDA competitive grants program. Baumgardt stressed the need for public funding for biotechnology as critical, but also pointed out its need to

⁷¹ Committee on Biotechnology, Emerging Biotechnologies in Agriculture Progress Report II. National Association of State Universities and Land Grant Colleges. November 1983. The Rifkin case involved the proposed use of a genetically engineered bacterium which would protect plants against ice damage. Normally plants can be injured by light frosts when surface bacteria serve as focal points for the development of ice crystals. The "ice-minus" bacteria lack the surface architecture of their close relatives and were thus unable to serve as an ice crystallization sight. If these bacteria coated the plants instead of the normal "ice-plus" bacteria, the plants would be protected from light frosts, a benefit critically important to certain agricultural crops. Obviously use of such genetically engineered bacteria would require field release and hence the controversy.

⁷² Minutes of the Committee meetings are maintained at the University of Florida. Judy Kite, Al Wood's secretary and secretary to the Committee throughout its early years maintained copies of them for IFAS administration and the NASULGC.

initiate greater unity between multidisciplinary biotechnology studies between university and industrial consortia. He argued at the same time for "a strong mission orientation" and "targeted basic research." The NSF university-industry research program was considered a viable model whereby federal funding was used "until the industries picked it up and was really the carrot to help encourage the industries to get involved to do their part."⁷³ The request for increased competitive grants funding was only partially met.

Later that year, in November 1984, the gold-covered "Emerging Biotechnologies in Agriculture, Progress Report III," was issued to continue the discussion. It was a natural outgrowth of the committee's original charge and interests. The introduction celebrated the newest breakthroughs in biotechnology, especially "the first instance of the transfer of a higher plant gene to another higher plant with expression of that gene in the new host plant."⁷⁴ The rest of the document dealt with the same political, economic and administrative concerns that had inspired its predecessors. Progress Report III contained a chapter entitled "Legal Framework for Scientific Inquiry at

⁷³ Baumgardt, B. Testimony. Hearings of the House of Representatives, Subcommittee on Agriculture, June 1984. p.14.

⁷⁴ Committee on Biotechnology, Emerging Biotechnologies in Agriculture, Progress Report III, 1984. p.2.

Public Universities," which "attempted to sensitize scientists and administrators to various components of this legalistic environment and provide them with a basis for interacting more effectively with elements of the private sector."⁷⁵ The sample contract provided earlier in the "Silver Bullet" was revised and updated as the result of review and comment from institutions across the country. Chapters on Social and Ethical Issues and, significantly, a chronology of the Rifkin lawsuit were provided.⁷⁶

An important outgrowth of the social issues previously raised was a chapter proposing a National Biological Impact Assessment Program (NBIAP). This was a move made to establish the pre-eminence of traditional agriculture (and USDA) in this area, to counter regulatory moves being made by EPA and NIH at this time. It declared: "... the traditional agriculture community should take the initiative in establishing protocols that would provide for the assessment of such impacts." The NBIAP "recognizes that agriculture has been assessing the impact of the release of genetically altered organisms into the environment for the last 75 years or so."⁷⁷ Although much of this rhetoric has

⁷⁵ Committee on Biotechnology, Emerging Biotechnologies in Agriculture, Progress Report III, 1984. p.2.

⁷⁶ Rifkin was suing Advanced Genetic Sciences to forestall their attempt to use the ice-minus bacteria they developed commercially.

⁷⁷ Committee on Biotechnology, Emerging Biotechnologies in Agriculture, Progress Report III, 1984. p.4.

been interpreted as self-serving and political, to many of the participants it also represented a real feeling that the mechanistic/bio-engineering principles and goals intrinsic to agriculture in the past were merely being recapitulated in the technologies of the present.

The 1985 report of this same Biotechnology Committee included a black-bordered "In Memoriam" to Wood: "Al's leadership was truly outstanding, and his personal vision contributed direction that few others could have provided. . . . Future beneficiaries of agricultural biotechnology owe a debt of gratitude to Al's superior efforts. Al Wood was a devoted scientist... and a true pioneer."⁷⁸ The rest of the document continued the tradition of survey data ("Industry Survey of Agricultural Biotechnology Research Development and Future Manpower Needs") and social policy and legal concerns. It was fitting that, along with further recommendations for progress of the NBIAP, an appendix was added detailing an example of an appropriate policy statement for the release and introduction of plant material into the environment. It was the policy statement developed in December 1981 by the IFAS Cultivar Release Committee, chaired by F. Aloysius Wood.⁷⁹

The "Silver Bullet" and the subsequent reports of the

⁷⁸ Committee on Biotechnology, Emerging Biotechnologies in Agriculture, Progress Report IV, 1985. Inside cover.

⁷⁹ Committee on Biotechnology, Emerging Biotechnologies in Agriculture, Progress Report IV, 1985.

NASULGC biotech committee had much of the desired effect. Partly because of its position statements, perhaps more so because of the survey data which was the first and most extensive of its kind on the current and planned state of biotechnology in the SAES, it was quoted everywhere, and continued to be recognized for its importance by the agricultural community.⁸⁰ It gained the Biotechnology Committee an apparently permanent life within the Land Grant System (it was still in operation in 1996, eleven years after Al Wood's death from cancer in 1985). But most importantly, the initial goals that it proclaimed were eventually achieved.⁸¹ This was partly because of their own lobbying efforts, but partly because of their prescience concerning the major push/pulls operating in agricultural policy.

USDA soon had its own official biotechnology arm and its competitive grants system was overhauled and expanded to favor biotechnology funding. By the late 1980's numerous public universities had established new biotechnology ventures and centers with the cooperation and funding of their respective state legislatures and with the help of the new competitive grants, even in times of fiscal constraint.

⁸⁰ Jordan, J. P. "Biotechnology: Promise or Pitfall", pp. 1-10., In: Valentine, F. A.(ed) Forest and Crop Biotechnology: Progress and Prospects (New York: Springer-Verlag, 1988).

⁸¹ Committee on Biotechnology, Emerging Biotechnologies in Agriculture: Progress Report VII, 1988. pp. 3-6.

The University of Florida, under Al Wood's guidance, continued its transformation. The idea of a biotechnology building evolved to the concept of dispersing new biotech faculty members throughout the departments under IFAS control. Al Wood took a personal interest in the hiring of each of the new faculty biotechnologists, even in late 1984 when the struggle against cancer was full upon him.⁸² The members of the early Recombinant DNA advisory committee he had formed moved into positions of authority and influence throughout campus.⁸³

Despite his early death, Al Wood lived to see part of the transformation he had struggled so hard to create. New dollars became available, new federal grants. Experiment station dollars more than doubled in biotechnology from 1982 to 1986 and biotechnology faculty at the land grant universities nearly doubled in that same period. The state of Florida, and others in the South especially, backed the biotechnology boom.

The much desired industry involvement in the university system flowered as well, nearly doubling from 1982 to 1986 as well, providing both financial promise and concomitant

⁸² Personal interview, November 1984.

⁸³ Curt Hannah became co-director of the Interdisciplinary Center for Biotechnology Research (ICBR) in 1987. Most of the others have achieved prominence in their various scientific disciplines (William Gurley and Indra Vasil already having a strong reputation for Agrobacterium and tissue culture research respectively).

fears for academic freedom that even today fuel the debate on the role of biotechnology in the future of agriculture. So successful was the transformation of land grant universities that, by 1985, top administrators at USDA/ARS were worried to the point of publicly expressing dismay at the proliferation of biotechnology "buildings" among the states, decrying, in a sense, their loss of central research control, all part of the groundswell shift that Al Wood represented.⁸⁴

USDA itself would ultimately not fare so well. Caught between political forces and internal rifts between social and production science in its own ranks, it was eclipsed at times by its SAES partners, and unable to take advantage of industrial support in the way that its state subsidiaries could. The federal organization began to lose much of its ability to compete for research dollars and prestige. Many of its attempts at self reform would be considered sham efforts and external mandates would come to be more and more important in guiding its destiny.

Eventually USDA found itself once again in "crisis" by the 1990s. As will be discussed in Chapter 8, biotechnology

⁸⁴ Excerpt of remarks by T. B. Kinney, Jr. Administrator, ARS BARC Symposium X Biotechnology for Solving Agricultural Problems, 1985: "Second, I suggest that recommendations for new biotechnology centers -- both at the Federal and the State levels may not represent the best use of our limited funds. Rather than investing in brick and mortar, I believe in putting the money directly into research." Research, presumably, under ARS direction.

would once more be trotted out as a panacea against all charges and complaints.

CHAPTER 6 "UNHOLY" ALLIANCES

Much has been made of the industrialization of biology that occurred throughout the development from molecular biology to biotechnology. This has been seen as a new and unholy alliance. The issues of academic freedom, scientific openness, dual-allegiance (especially with regard to public/private responsibilities) and even fears of a corporate "brain drain" fueled the debate in the popular press, the more "prestigious" policy journals and, more importantly to this chapter, the offices of scientific administrators and researchers.¹

In agricultural science, however, there were peculiar ramifications not present in the simple claim that they were "selling-out" the university's intellectual "cachet." For more than a generation before biotechnology was an issue, the development of a corporate capitalist approach in agriculture had begun a process of transforming the "client" in agriculture from family farm or small growers' alliances

¹ See Teitleman Gene Dreams (1989), and Kenney Biotechnology: The University/Industrial Complex, (1986), among others.

to corporate giants and international markets.² This connection is seen as obvious, and yet biotechnology (because of the revolutionary aspects of its abilities and the even more revolutionary aspects of its claims) is seen as fundamentally different. The technology was indeed different, but for agricultural science and industry, at least, the goals were the same.

Researchers such as Martin Kenney give cogent analyses of biotechnology and the university/industrial complex, but are most accurate when dealing with the non-land grant universities. Because of reliance on large scale organizations and on industrial biotechnology as the main focus, Kenney, who rightly points out that the LGUs appeared to have gotten into the fray late in the game, misses much by not realizing the tremendous amount of "on-the-ground" maneuvering well before the major shifts were apparent.³

Neglecting such behind the scenes preparations makes Kenney and others too sensitive only to external forces on

² Corporate capitalism is an outgrowth of the late nineteenth century when increasingly large business conglomerates created near monopolies and replaced the "small business" enterprises that were the hallmark of earlier forms of capitalism. According to Dawley (1991) modern American "big" business developed as "competition [turned] into managed markets, and hit-or-miss methods turned into rationalized production, the competitive capitalism of the nineteenth turned into the corporate capitalism of the twentieth.(p.33)" Developments in agricultural industry mirror this transformation in American business as a whole.

³ Kenney, M. Biotechnology: The University-Industrial Complex, 1986.

agricultural science for the most recent changes. These authors try to recapitulate the more external, financial aspects of the crisis in the '70s as a key factor, assuming that outside agencies forced agriculture to take new account of itself and join with industry. In his 1986 book, a classic analysis of the development of industrial biotechnology per se and the non-LGUs, it was too early to realize the depth of change taking place at the LGUs.⁴

Kenney dismissed as a mere "survey" the NASULGC Biotechnology Committee report (the aforementioned "Silver Bullet") that I find critical and telling. He chose to focus on a 1982 Winrock document critical of agriculture as a more cogent force "shaming" the LGUs into action. This focus neglected the fact that the LGUs were not wary or fighting the change, but rather were desperately trying to get and shift funds from their traditional bosses and to forge even greater alliances with industry all on their own.⁵

There may be deeper reasons for the assumptions of an academic innocence that was then "corrupted." Much of the opposition to agricultural biotechnology's new alliance with

⁴ Kenney, at this point had no access to archival material from, nor behind the scenes contact with, those very people, such as Al Wood and John Fulkerson and other members of the NASULGC Biotechnology Committee, who would tend to ignore sociologists.

⁵ They were still, in fact, responding to the criticisms and crises of the '70s portrayed in Chapter 3 of this dissertation, and to the new political situation in funding and the changing structure of the federal government with regard to agricultural regulation.

industry would come from the new sciences, quite foreign to traditional "scientific farming" concerns. The rural sociology profession and positions that developed in the Great Depression and post-depression period, were in fact not responding against the new biology itself, but against it as a manifestation of this already increasing industrialization process.

Using a professionalization model such as that applied to American medical practitioners by Kenneth Starr⁶, these critiques can be seen as a move toward professional autonomy and authority in USDA on the part of the social sciences. These social science possibilities were created by external forces, in particular by the congressional and presidentially driven expansion of the role of USDA into urban and societal affairs and away from solely production agriculture.⁷ In one sense, the vilification of the

⁶ Starr, The Social Transformation of American Medicine, 1982., presents the typical model of professionalization whereby group identity is consolidated by the creation of common agendas, development of disciplinary organizations and paradigms, and by alliances with and against client groups and perceived enemies.

⁷ The Great Depression inspired legislation to deal with farm surpluses by decreasing production through a series of agricultural adjustment acts. Rural assistance programs were initiated and expanded. Through Great Society legislation in the 1960s, USDA became responsible for major foodstamp programs for urban inhabitants on top of the commodity subsidies that existed to benefit farmers and farm industries. Social workers and sociologists became ever more involved in food and nutrition issues, in the welfare of both urban and rural poor. Corporate industrialization of agriculture increased calls for benefits to small farms and "family farms." This culminated in attacks on LGUs

biotechnology university-industry axis can be seen as a response by the new sociologists in defense of what they deemed "their clients" against what has traditionally been seen as oppressive global industrialization. The sense of newness in the profession of this industrial mentality was, in part, an assumption of a prior "good conscience" on the part of their "hard science" colleagues in agriculture, and in part a function of when these researchers entered the picture.⁸

The rhetoric of the "family farm" and rural development would provide a continued counterpoint to the larger economic forces driving agriculture throughout this period toward greater economies of scale. It exacerbated a profound

participation in such industrialization (Hightower, Hard Times/Hard Tomatoes, 1973) and led to legislation that highlighted rural sociology issues and protection/promotion of rural and small town life. Hadwiger (The Politics of Agricultural Research, 1982) pointed out that USDA official "Parlberg later coined the term "new agenda" to suggest that the concerns of commercial agriculture had been replaced by the heretofore neglected concerns of and for small farmers, farm workers, the poor, racial minorities, consumers, and the environment.(p.9)" Such "new agenda" items would become incorporated into the 1977 Food and Agriculture Act "united with the continuing agenda of servicing U.S. commercial agriculture.(p.11)" It can be argued that much of the subsequent rhetoric against biotechnology would be from "new agenda" supporters, where the majority of its advocates would be of the traditional commercial agriculture and production-oriented research establishment. A de facto civil war.

⁸ One might posit that the problem on both sides resulted from the lack of historical context for any but their own disciplines, their own mythologies and their own client histories. But then this is one of the characteristics of professionalization -- a requirement to consolidate loyalties and simplify goals and procedures.

stress fracture in the very mythological foundation upon which American agriculture was based and upon which USDA had built its house.

USDA's traditional non-social research arm was caught in a realm of now conflicting priorities. No longer did the democratic, agrarian virtues as embodied in the family farm seem compatible with Eden's goal of continually increasing food production and food for all. Additionally, the science and "scientification" to which the agricultural research administration had committed itself, especially after WWII, was developed almost solely with efficiencies of production made possible by economies of scale in themselves contrary to the family farm. Politically and economically, the traditional, production oriented research establishment found itself having to defend its increasing abandonment of what was, in numerical reality, a diminishing clientele group in both actual numbers and economic clout. Industrial pull was not needed to instigate many of the changes that this diminishing made obvious.⁹

⁹ Pushes made by LGUs towards expanding their urban support base were typical. The University of Florida followed a well-established pattern. It sponsored various "plans" to transform research priorities into ones at first not counter to, and then fully supportive of city consumers. Their old clients were to be trained how to adapt to the growing clout of the new. This was one of the fundamental premises behind addressing Agricultural Growth in an Urban Age (IFAS, 1975) and the development and institutionalization of fields such as Urban Wildlife and Urban Forestry at the University of Florida, School of Forest Resources and Conservation in the 1980s.

Some of the first complaints from scientific and cultural critics in the 1970s were responses to the LGUs first attempts to solve this problem with a greater focus on industrialization. Biotechnology would prove to be new wine in old bottles, as far as this process was concerned.

Don Hadwiger (1982) listed six categories of private groups that make up the industrial section of the government-university-industry triangle.¹⁰ The general farm organizations tended to provide the counterpoint to the production mentality and represented part of the internal problems faced by the industrialization of agriculture per se and the long-term co-opting of agricultural research by agribusiness. The National Farmer's Union (formed in the early 1900s), the National Farmer's Organization (formed in the late 1950s), and the American Agriculture Movement (formed in the late 1970s) provided a triple problematic both for agricultural research and for the view that biotechnology was the key purveyor of industrialization into the universities. Their fears and complaints were in place well before genetic engineering concerns. As Hadwiger points out:

All three groups have ambivalent views toward agricultural research: they recognize that it provides

¹⁰ Hadwiger, The Politics of Agricultural Research, 1982. p. 90. These were: "general farm organization, agricultural trade organizations, farmers' cooperatives, agribusiness corporations, and agency groups associated with a particular program, such as the Association of Soil Conservation Districts."

remedies against disease and blight and makes American products more competitive in international markets, but at the same time, they blame agricultural research for having produced the surpluses which hold prices down and therefore benefit consumers and agribusiness rather than farmers.¹¹

As mentioned before, the one counterweight to this complaint about agricultural research was The American Farm Bureau Federation, whose growth, as Hadwiger details, was entwined with the state extension service. These farmers tended to run larger and more conservative businesses, but also were more indoctrinated by extension, which served in a very real sense as the educational and local political arm of the agricultural research system.

Commodity groups provided the key powerbase for the defense of the agricultural research system and its funding nearly from their inception and these tended to be industry wide.¹² Commodity organizations provided the perfect partner for agricultural research as it had always been done. Theirs was "a bullish posture that welcomed new technology, in contrast to the old strategy of reducing production in order to increase prices and profits."¹³ As the farm research

¹¹ Hadwiger, The Politics of Agricultural Research, 1982. p. 92.

¹² Hadwiger, The Politics of Agricultural Research, 1982. p. 93. "In many groups, producers or farmers are not longer influential . . . because their economic role in the industry is now relatively small and . . . processors or others are the more aggressive supporters of the commodity organization."

¹³ Hadwiger, 1982. p. 94.

system was biased toward commodity directed research (as are farm subsidies and congressional legislation) commodity groups were deemed most influential by Congressional staffers when polled about private group influence upon agricultural policy (both general and research policy) in 1979. General farm organizations were least influential in determining research policy, below trade organizations and agribusiness.¹⁴

But these traditional beneficiaries of farm research found their voices diluted as the late 1970s and 1980s brought new players into the picture. As pointed out in recent discussions of the change in farm policy and farm legislation, USDA and farm bill production were increasingly at the mercy of a fragmented client group, in part created by Congressional "diluting" of the original USDA mandate by the addition of social responsibilities, including Food Stamp Programs and Rural Development.¹⁵ Civil rights in the broadest sense, including the rights of migrant laborers and minority farmers, contributed a factor to the equation of

¹⁴ Polling was done by Paul Gardner for Successful Farming Magazine. Hadwiger, The Politics of Agricultural Research, 1982., presents the results of the poll in text and table on pp. 94-96.

¹⁵ See: Peters, J. G. "The 1977 Farm Bill: Coalitions in Congress," pp. 23-36, In: Hadwiger, D. F., and Browne, W.P. (eds) The New Politics of Food (Lexington: Lexington Books, 1978), and Bonnen et al. "Further Observations on the Changing Nature of National Agricultural Policy Decision Processes, 1946-1995," Agricultural History 70(2): 130-151, 1996.

farm productivity which was completely outside the standard productivity analysis. The rise of industrial labor rights had also 'contributed unexpected "costs" to industrial production in an earlier era.

A sense of moral decline in the country as a whole, fueled by the 1960s and post-Watergate cynicism, coupled with the rhetoric of a Reagan administration lambasting urban depravity and calling for a return to "family values" and a mythic American past, helped to reinvigorate the concept of the noble agrarian. In addition a real shift in economic viability of the small farm v. its corporate competitor was fueled by international market forces, costs of credit, and the politicalization of food policy begun in earnest by President Carter. This created a developing sympathy for the "little guy" that would provide ammunition against the new biotechnologies as it had against the industrialization of technology from the beginning. Thus the disproportionate influence of commodity groups and agribusiness on research came under attack from a new urban powerbase and from "new" professional groups forcibly thrown into the research mix by various social legislation and the 1977 Farm Bill.

Rural sociologists would discover in small town America and in the family farm an endangered culture that would require preserving. This culture possessed an innate value and integrity similar to that of other endangered cultures

in so-called third world countries. An unsympathetic read of the motives of sociologists in this period would detect a move to advance the importance and political utility of "their" clientele at the same time as they moved to assert their developing power in Congressional committees and presidential politics. A nostalgic urban populace also took solace in the myths of an agrarian Eden. The "New Populists" had academic champions and hired-guns in the core of USDA--a far cry from the situation in the 1890s.¹⁶ To USDA's chagrin, many of the people charging unholy alliance were on its own agricultural science payroll--as part of social science projects mandated from above.

What did this mean for biotechnology and for traditional scientific research both in USDA and as it filtered to the land-grant institutions? In one way it meant a new series of yearbook titles reflecting the new categories of emphasis.¹⁷ But in a more fundamental way, it meant a shift in funding strategies. The privatization pushed by the Reagan administration as a returning of

¹⁶ One of the reasons for the failures of the original Populists lay in their lack of support within the political and academic establishments.

¹⁷ See, for example the environmental concerns of the 1983 USDA Yearbook Using Our Natural Resources, and the 1991 Yearbook Agriculture and the Environment. Farm issues were addressed and tied to urban concerns in the 1982 Yearbook Food- From Farm to Table, and the new concern for markets and a global economy were reflected in the 1985 Yearbook Agriculture in a Global Economy and the 1992 Yearbook New Crops, New Uses, New Markets.

government to the people, was, in theory to benefit small business and the traditional capitalist virtues of a by-gone era. In practice, it enabled an even greater shift to corporate capitalism by a research establishment that had already committed itself to increased production and to every more high-input/high-technology agriculture.

A significant component of USDA, now faced with urban responsibilities, newer definitions of global competitiveness, and food production as an economic defense industry, was able to deify the consumer over the farmer. USDA would re-iterate production-oriented goals in the rhetoric of lower prices for the consumer and better market capture from the world. Again, the issue was the same as with the Populists from an earlier era--overproduction was the bane of the small farmer, the delight of the large-scale producer and the consumer. Faced simultaneously with a decreasing funding base, internal attacks from its own rural sociologists who incited and took advantage of the mythic sensibilities of the public at large, research administrators in the production disciplines took the logical course of embracing an industrial clientele and an urban majority. The development of biotechnology meant that a new source of corporate funding could be incorporated into research strategies at the same time as dismissing the concerns of many of their internal and external foes on the basis of science and progress once more. Biotechnology could

transform the bad image of corporate alliances into the good one of privatization, national competitiveness and consumer welfare.¹⁸

It is obvious then, why rural sociologists would be quick to disbelieve the possibilities of biotechnological "fixes" on the system. They were fully aware that this was only peripherally a friendly technology to smaller interests. It was, as USDA production researchers intended, a way of making industrial-scale productivity more palatable to an urban public ultimately more worried about pollution, food costs and national security than about the "family farm" and migrant labor.

It was not the creation of an unholy alliance, or if it was, it was the re-creation of it. The goals of scientific farming had always been increased production. The history of technology points to the almost inexorable increase in scale and industrialization necessary to push productivity ever ahead. Ultimately, the hedonistic aspects of the myth of Eden were the most important in promoting "scientific farming" if not the political goals of USDA.¹⁹

The availability of an infinite and easy food supply, not the moral virtues of Adam, would ultimately be the

¹⁸ It is telling that biotechnology was routinely held up as the panacea to solve all these problems.

¹⁹ Hadwiger, The Politics of Agricultural Research, 1982. p. 92. pointed out, even in 1982, the year of the Silver Bullet's conception: "Most economists agree that farmers are not the major beneficiaries of research."

selected goal. This Eden was deemed for everyone, not just the rural farmer. The fact that its long-term development involved partnership with a viable source of research funds, choice of a professionally acceptable research technology and affiliations with "friendly" clientele who provided ego-massage as well as money, was an added bonus, to be sure.

The industrial shift in agricultural research, so pronounced in the development of modern technologies, was itself an evolution, not a revolution. The attitudes and negotiations among university research administrators and government officials throughout this period were not the strained friendliness of wary adversaries seeking a common goal, but almost the genuine delight of long-term partners realizing a new and potentially more profitable path for increased interaction. Real fears did exist, because their goals were not wholly compatible, of course. Industry and the university both ideally desired polygamy or polyandry on its own part, monogamy on the part of its associate. Industrial research goals, for example, were at times a bar to academic advancement with regards to delays in publication due to patenting requirements.

Where internal institutional difficulties existed, biotechnology could help to smooth these out. A crisis within NASULGC itself occurred in 1979 when the university presidents (seeking to serve the interests of both agricultural and non-agricultural professionals) and the

agricultural research administrators (in the experiment station and extension committees) split on budgetary concerns and lobbied separately.²⁰ By the time of the "Silver Bullet," biotechnology could provide a mutually palatable budget anodyne that NASULGC as a whole could support and one which Congress would readily swallow.²¹

At no point was there a breach with the developing industrialization of agriculture that had been in existence from agriculture's first mandate toward manufacturing and production oriented research. The industries grew larger, family farms grew less, and the university research community continued to deliver research "product," an industry itself. The fears that social analysts such as Lawrence Busch, Martin Kenney and others expressed were somewhat at odds to the historical developments in agriculture. Fears were raised that, because of biotechnology, "the university, a peculiar and fragile social institution that can trace its history back to feudalism, is being subsumed by industry, one of the very institutions with which it should, in some degree, be in conflict." Claims were made that "when university and industry become partners, the entire society is

²⁰ Kenney, 1986.

²¹ Land Grant University presidents could also accept biotechnology funding as a growing part of the budget, in part from fear of competition from their non-land grant sister institutions which were benefiting greatly from Wall Street investments in biotechnology.

endangered."²² These fears and claims, whether applicable to private universities, neglected a recognition that, in the broadest sense, industrial complicity was embedded in the nature of public agricultural research and the land grant universities from the start. Individual farmers, since the Populist era had been "victims" of overabundance designed to benefit consumers and industrial participants in growing world markets and a developing corporate capitalist system.²³

There were significant industry-related problems with the development of biotechnology to be sure. Newer sources of money were available and the ability of researchers to profit more than through prestige and institutional rise became possible for the first time. This led to fears of financial conflicts of interest.²⁴ What would confuse the public and many sociological analysts later and make them blame the "new" industrialization, was the fact that such financial dealings created scandals which opened a window on a system that was itself formerly protected by myth. By the time of the rise of biotechnology it was a myth that

²² Kenney, Biotechnology: The University-Industrial Complex, 1986 p 246.

²³ See Summers, M. "Putting Populism Back In: Rethinking Agricultural Politics and Policy," Agricultural History 70(2): 395-413, 1996., for a sympathetic read of populist resistance to the egregious capitalism of big business and corporate industrialization.

²⁴ Hart, K. "Is Academic Freedom Bad for Business," Bulletin of the Atomic Scientists April, 1989. pp. 28-34.

production-oriented agricultural research had anything to do with the family farm and even "farmers" per se. Outside observers (and the new sociological "insiders") suddenly discovered the unholy alliances that had been developing all along.

Even intelligent analysts such as Busch and Lacy, in their Plants, Power and Profit who were fully capable of pointing to the precursors of this industrialization as inherent in the system,²⁵ and Jack Kloppenburg in his First the Seed who saw the development and control of seed technology as the first step toward biotechnological industrialization,²⁶ somehow still saw biotechnology as "different" in its scope. The difference, as Kenney pointed out, was that non-land grant university scientists, and scientists not normally involved in what would be considered "industrial" research would be pulled in to the industrialization of biology.²⁷

But agriculture had been there all along. Agricultural researchers, especially in USDA and the land-grant

²⁵ L. Busch, et al., Plants, Power and Profit: Social, Economic and Ethical Consequences of the New Biotechnologies (Oxford: Basil Blackwell, Inc., 1991) p. 65., pointed out the interest of private industry in the early development of plant hybridization techniques.

²⁶ J. Kloppenburg, First the Seed, 1988, p. 132-135, details how the Plant Patent Act of 1930 was a move strongly towards the industrial transformation of agriculture.

²⁷ M. Kenney, Biotechnology: The University-Industrial Complex, 1986. sees the entire biotechnology industry as forging this alliance.

universities had followed a quasi-industrial mode from the very beginning in their necessary political responsiveness to grower's groups and individual farm lobbies as their client/bosses. But even more so, in their ultimate commitment to food production as a universal research good. Busch et al. especially place what they call the "demand" for new biotechnologies squarely on economic factors, and somewhat naive ones at that. The demand "emerged out of changes in patent law, the decreased funding of SAESs, the interests of chemical and pharmaceutical companies. . . . and perhaps the desire of (some) farmers to compete more effectively in world markets."²⁸ This conclusion totally ignored both the research ideology of the SAESs that drove them toward the development of biotechnology even in the heyday of post WWII funding prosperity, and the real fears of these institutions that their mission was failing on global paradigmatic grounds. High-input agriculture was under attack, theoretical yields were reaching plateaus, energy crises and pollution were growing problems, scientific prestige was at a low ebb, and world hunger loomed on the horizon.

Of course, patent issues with industry were thorny ones, and yet patents were immensely more attractive, especially with the new laws permitting public universities

²⁸ Bush et al., 1991. pp. 191-200.

to profit on research.²⁹ Greater ties with industry were problematic due to differences of research style: the industrial need for continued "progress reports" and managerial input, compared to the university scientists historical tendency to report only upon completion, and to have only final, not continual accountability. But these issues were far less thorny ones to agricultural researchers than might be imagined. As the first of the "big sciences" in terms of agricultural funding, numbers of interacting scientists, and political bed-fellows, the agricultural establishment was far more in tune with industrial research models. And, with the developments of agricultural technology from mechanical to chemical, USDA and the land-grant industries had been intimately tied to the enhancement and productivity of American agricultural industries.

Biotechnology infiltrated a system of long-term relationships and friendships. To decry this move as something caused by biotechnology or the loss of government funding or as a "betrayal" of long-held academic ideals on the part of the university/government agricultural establishment is to be disingenuous and ahistorical.³⁰

²⁹ Committee on Biotechnology, 1983. pp. 47-51.

³⁰ It may be part of a rhetorical stance for some critics of the current situation, who by rewriting this historical story would create an idyllic rural past that it is desirable and possible to recreate, but the documentation, at least as far as the land-grant university system is concerned, seems very much against this image. The "rightness" or "wrongness" of the current situation must be

As shown in the previous chapter, from the very first industrial support and biotechnology funding concerns went hand in hand. The goals of many of the original biotechnology investigations were industrial goals. This was recognized and condemned by many if not most of the critical reviewers of the development of modern agricultural biotechnology, but it was also seen as a betrayal of previous allegiances. Such a view misreads the fundamental dichotomy of agricultural research and the role of USDA from the beginning--food production was the goal and the good pursued. Farming and farmers were ultimately seen as mere means to that end.

Misreading of the moral dichotomy creates both a villainization of the new biotechnologies and a misrepresentation of history. It is responsible for seeing a newness in biotechnology that is not there, despite the glamour of its potential. It is in part a reflection of the mirror-image allegiance of the newer and politically grafted research arm of USDA and agricultural science: that of the rural sociologist, farmer-activist and "values" nostalgist. The new biotechnologies are symptomatic in agriculture of deeper problems in the social fabric where industrialization itself is taken to be problematic.

For many critics of biotechnology, farmers and farming

judged on moral grounds, not on fidelity to an arguably non-existent historical antecedent.

were the intrinsic good, and food production instead the means to the end of a healthy family farm and rural values.³¹ This trenchant split in values is at the core of the modern historiography of biotechnology in agriculture, and the fundamental fissure across which policy debates are held both in academia and in congressional subcommittees.

This dissertation claims that both histories are valid in the political mythology of US agriculture from the very start. The supposed value neutrality embedded in concepts of modern science permitted "scientific farming" to have only one road to success, one road to progress, one reliable, unquestionable measure of improvement--the dream of two blades of grass growing where only one grew before. The American Eden would be the breadbasket of a hungry world, in this vision, not the happy home of a self-sufficient, agrarian Adam.

The other side of the vision is most easily seen in the

³¹ The rhetoric of protecting the family farm remains powerful. Rural and small town values retain a nostalgic hold on American life. In Bird, et al. Planting the Future: Developing an Agriculture that Sustains Land and Community (Ames: Iowa State University Press, 1995) the issues seem clear: "The U. S. system of agriculture is the basis of American civilization. . . . Farming . . . helped establish democratic principles, helped overthrow European control . . . and helped settle every state. Those who care about where America is heading are wise to recall where we came from. . . . Will only 4 percent of American farms produce 75 percent of all U. S. farm products as predicted. . . . Or is there an alternative view of the future of American farming?" (p.4). Concern as to whether an alternative is needed is at the core of the debate between the two views of Eden. Where one sees loss, the other sees economies of scale.

original Populist era, when many small farmers first began to recognize that production was not intrinsically good for their economic well-being. It was a long, painful lesson for the farmers themselves, who were driven (at least the smaller farmers) to accept the hollowness of the production myth by harsh economic reality.

But there was no impetus for the agricultural scientists to abandon that particular myth. It was the myth that propelled the successes of the corporate capitalist restructuring of farming and made these farmers (large scale or corporately organized) the chief clientele of the American research establishment. USDA and the land-grant system cleaved to the American Farm Bureau with its production mentality from the start, not the farmer federations of populist ilks who would, in the modern era push for alternate technologies and controlled production. If, and ultimately when, overproduction was seen as a problem by these conservative forces, the solution was not to shut down the factory in Eden, but rather to create new markets, whether by increasing global trade or by the development of "new crops/new uses/new markets."³²

The traditional agricultural research establishment as a philosophical whole never abandoned the belief and the rhetoric that began with the establishment of USDA and even more characterized post-WWII farming. To quote Clinton P.

³² The title of the USDA Yearbook for 1992.

Anderson, Secretary of Agriculture (1947) is to quote Al Wood. It is to quote the Farm Bureau and the adherents of "Science in Farming" from the beginning to the present. It is to quote the counter-revolutionaries who embraced biotechnology, as the means to these goals, in response to those who would have shut down progress and production.

Can we never be satisfied - must we go on with research forever? Does not this technology lead sooner or later to overproduction? On such points I have no fear: We did not stop making automobiles for fear we would wreck them; or leave off erecting dams lest they burst; or refuse to construct houses because they might cave in. And need we be concerned that life be too abundant, that we and others in the world have too much good food, too many clothes, too many medicines for our ills, too much leisure to look upward? Rather let us give thought to getting food to people who need it, feeding ourselves and our neighbors better, putting farm goods to further uses in industry, taking care of our land, trading willingly and freely, and cooperating effectively to maintain full employment.³³

Anderson's view ceased to be the message from those who advocated production curbs, boycotts, a return to "ecology-friendly" and production limited family farming. It was a global message in an increasingly protectionist society. And, to be sure, it was the message of global corporate capitalism against the more "mom and pop nationalism" of the family farm. It was the message of industry and the federal government with its concern for foreign markets and balance of trade, the message of city consumers with their demand for more and cheaper food. It was anathema to rural

³³ Anderson, 1947. Science in Farming, p. v-vi.

sociology, farm laborers, and the family farm -- as much so as Taylorization and the assembly line was the death knell of the powerful craft union and the small shop.

How, then, did the developing forces of biotechnology in land-grant agriculture relate to and adapt to industry? For different reasons, the long-term goals and faith in progress was the same. As physicists cleaved to the Department of Defense with their consciences clear that the USSR was the demon and the cold war was a just war, so did the "progressive" scientists and researchers in agriculture cleave to industry as the arm of greatest food production, of more and cheaper food for all the world. As with the physicists, many traditional agricultural scientists would see their so-called unholy alliance as necessary for another, and perhaps even higher, good as well: that of doing "good science" when no one else was there to pay for it. The Reagan administration's actions from 1980 on not only encouraged ties to industry, they forced it as a matter of budgetary principle through cuts and matching funds programs dedicated to developing university/industry research consortia as part of the goal of budget balancing and, more so, privatization of federal aims and programs.

It was thus even more understandable that agricultural research adapted quickly to these changes and clung to industry, forging even greater ties. They had never really been apart--as much of the complaints against USDA and the

land-grant system had pointed out in the 1960s and 1970s. So too, the dawn of industrial interest in agricultural biotechnology had not begun with Genentech. Industry had been paying for much of the tissue culture, pesticide and herbicide research that would lead to the so-called new biotechnologies ever since WWII. As shown, the archival record is replete with information on the friendly and increasingly intimate ties of industry to land-grant academia inspired by the new biotechnologies.

Industry was not perceived as a perfect partner, by any means. But, due to the fact that agricultural research was "used to" industrial partners and industrial concerns, the land-grant university system tended not to have the same kind of "paranoia" about industrial destruction of traditional academic freedom. The intrusion of industry was not that of an alien, but rather the increased familiarity of a neighbor. Everything was done to welcome and facilitate interaction, and academic concerns were to be protected by negotiation and compromise, not through rejection or confrontation.

Fundamentally, the attitude of the agricultural administrators was that financial "conflicts of interest" were to be avoided. Such problems were indeed increasing rapidly at the time due to the involvement of research faculty in start-up biotechnology companies. But for the agricultural institutions, full disclosure of all "outside

professional activities" was the key, not preventing closer industrial ties. Charles Hess, Dean for Agriculture at the University of California Davis and Associate Director of the Agricultural Experiment Station, stated in Congressional hearings on Commercialization of Academic Biomedical Research in 1981, that the situation was different at the LGUs.

Land grant universities such as the University of California at Davis have a long history of a close working relationship with the private sector--agriculture. Congress in 1862, recognizing the importance of a viable agriculture to the nation, endorsed the concept of linking teaching and research with the private sector by establishing land grant colleges.³⁴

This attitude was in keeping with the production oriented mentality of agriculture from the beginning. Hess, in his testimony, refers to the January 1980 issue of Science in which the Assistant Director of the Office of Science and Technology Policy discussed such interactions in a positive manner, saying that "good university-industry relations help insure that research is relevant to national needs and bring about a more rapid application of new knowledge to solve societal problems, thus helping to increase productivity."³⁵ Hess would go on to become one of the founding committee members of the NASULGC biotechnology committee, working intimately with Al Wood.

³⁴ Hess, Testimony, 1981. p. 73.

³⁵ Hess, Testimony, 1981. p. 74.

Apparently, the main fears of industrial moves into biotechnology and the concomitant relationships with the land-grant universities other than the conflict of interest issue (which seemed, at least to men like Hess and the others involved in the NASULGC, clearcut) were three-fold:

First, by providing higher salaries and benefits such as stock options and enhanced research facilities, industry would create a "brain" drain on the relatively limited pool of biotechnology researchers then available.

Such fears of insufficient biotechnology researchers were not used as an excuse to attack industry, but were instead used as one of the key reasons given for demanding increased government financing for laboratories and salaries capable of maintaining public sector attractiveness. At the same time the LGUs would push for a massive investment in training of new biotechnology researchers -- enough to eventually fill both university and industrial demands. Teaching and training of graduate students and post-docs was a critical component to any scientific enterprise, not only for the subsidized "slave labor" and the intellectual "reproduction" aspects, but for the simple reason that it was the job of universities to turn out students.

A second fear involved patent issues, which were still in a nebulous legal realm. University legal staffs, it was assumed, were not up to the job of providing sufficient contractual protections to gain an appropriate share of

profits -- thus standardized guidelines were needed. It was only since 1980 that Congress permitted the land grant universities to fight for their share of the pie, and though eager to do so, they were also new at it.³⁶

Finally, there was a worry that industry might interfere with the timely flow of academic information regarding professional publications, thereby impeding both the collective research and individual research careers of university scientists. This latter concern was especially voiced by private universities under the rubric of academic freedom. In the discussions of the public research administrators however, it seemed to be an issue more of timing interests, not a moral or ethical concern about intrinsic academic freedoms.³⁷

The section on academic freedom in Legal Framework for Scientific Inquiry at Public Universities, the keynote document on the subject by the Committee on Biotechnology of NASULGC contained the rather chilling conclusion that academic freedom was inherently limited and industry was a legitimate source of such limitations when funding issues and state concerns were involved. For example, these rights were seen as limited "by the conditions of employment of the faculty member, the general mission of the university, [and]

³⁶ Hess, Testimony, 1981. p. 75.

³⁷ Hess, Testimony, 1981. p. 75.

the explicit expectations of the funding agency. . . . Even though free expression and information gathering are guaranteed by the First Amendment, these rights must often yield to other important state interests."³⁸ The discussion then became an analysis of "police powers" that limited such freedom based primarily on safety issues, a necessary nod toward protecting the public from dangerous genetic experimentation.

By the 1980s and the flowering of the "Reagan Revolution", the rhetoric that tied corporate interests to public interest, the notion that the good of "business" was the good of the government had pervaded Congress to the extent that governmental funding reflected a bias in favor of industrial exploitation.³⁹ Land-grant institutions in their public role accepted industrial partners willingly as part of a continuing re-definition of "clientele" and thus did not see it as a moral abrogation of their duty to society at large. Publication issues were thus more about professional questions of career and research program development than the sanctity of the ivory tower. Such questions were more typical of private institutions, whose

³⁸ Committee on Biotechnology, Legal Framework for Scientific Inquiry at Public Universities, 1987. p. 8. This document was started in 1983 and vetted throughout a host of lawyers and land-grant university research administrators and SAES directors.

³⁹ Many start-up grants were designed to enhance industry/government partnerships by offering matching funds to projects co-sponsored by industry.

industrial involvements had been comparatively limited to this time. Most telling was the section in the Legal Framework document which dealt with "The Public Purpose Doctrine." Here was absolute validation of the Reagan vision of privatization and trickle-down benefits.

The concept of public purpose is quite easy to state: public funds are to be utilized only for the benefit of the general public. However. . . the dividing line between public benefit and private benefit becomes very hazy. Private individuals or associations always benefit from a public expenditure or improvement. This reality does not necessarily detract from the public nature of governmental action, as long as there is substantial benefit to the public. The proscription inherent in the public purpose doctrine is against risking public money or credit for the primary benefit of private parties.⁴⁰

Obviously this was not the manifesto of an academic organization under siege by industry demands, but rather the consensus of a conservative agenda that saw the good of business as not only compatible with public good, but a major cause of it. This was the same attitude that allowed physicists and engineers to support massive defense industry spending with the concept of "public good" via national security as the ameliorating excuse. So too were the issues of "food security" and better science the public goods which allowed for a greater intercalation of the land-grant agricultural research sector with its already extant industrial friends.

⁴⁰ Committee on Biotechnology, Legal Framework for Scientific Inquiry at Public Universities, 1987. p. 6. Emphasis in original.

In none of the administrators' documents can one detect anything but a profound respect and desire to elaborate interactions with their industrial collaborators. The motive was more than just financial hunger, it was also the absolute belief that industry had historically been and would continue to be one of the major facilitators of university discoveries translating themselves into public good. As the document concludes, the benefits of the industrial partnership were magnified:

Since... Diamond v. Chakrabarty, private industry has strengthened its relationships with universities in order to tap the swiftly developing pool of information These new relationships offer promise of a better quality of life for many people. . . . These advances can be spurred on by an influx of private financing, especially when government funding is limited.⁴¹

Certainly from the very start the NASULGC Biotechnology Committee was involved with industrial relations as much if not more so than it was with the rest of state and federal government. As part of official committee activities in 1983, Wood and Fulkerson met with Dr. Ralph Hardy of DuPont (April 27), Wood toured Monsanto's biotech facility (May 2-4), Wood met with Dr. Bill Marshall of General Foods (June

⁴¹ In 1987, Legal Frameworks was considered important enough to reprint in its entirety from its original printing in the 1984 Emerging Biotechnologies report. This selection is from p.37-38 in the original report, p. 55 in the reprint. In both cases it also included a discussion of the importance of research and development tax breaks as an additional stimulus to the developing university/industry relationships.

7) and Wershow gave a Seminar on Biotech Social and Ethical Issues, sponsored by the Industrial Biotechnology Association (June 21-22).

Issues of ethics would become critical to the discussion. Members of the NASULGC committee considered it critical that the land-grant university sector act to allay public fears. Recognizing the LGUs role as trusted experts, Chairman Wood was willing to trade on this cultural cachet, using the university to smooth the way for industry. In a letter to the full Committee on Biotechnology, dated October 25, 1983, he suggested that academic association sponsorship of a meeting to address the areas of ethics and morals of research and development in biotech "would be more readily accepted by the lay community. . . . Also it would result in an informed citizenry with an understanding of the pros and cons - mostly pros - of the application of this technology by industry."⁴² This was stated as being in full accord with the view of John Fulkerson, CSRS. These were not the views of men being led unwillingly into the arms of industry, nor was it atypical of agricultural science's response to their well-known and well-respected partners in corporate America.

As mentioned, one of the few negative notes raised in this relationship could be seen in a NASULGC questionnaire

⁴² Letter from Al Wood to Committee on Biotechnology, dated October 25, 1983. (Gainesville: University of Florida Archives).

from July 6, 1983, specifically asking the LGUs about losses of biotech personnel to industry. Far from the LGUs being seduced by industry, it was industry that was often the less than enthusiastic partner. Its options for farming out research were rapidly increasing with private universities and specific biotechnology development firms clamoring for the traditional agricultural industry funding sources like General Foods and Weyrhauser.

Industry representatives often seemed unimpressed by the agricultural sector's attempts to recapture their share of the funding pie. Bill Marshall called into question the biotechnology committee's assertion that there was an across the board decrease in federal research dollars. For the General Foods' executive it was enough to point to the increases in NIH and NSF funding for basic research and biotechnology. They were on a remarkable increase, Marshall stated in a letter to Wood. Only USDA was receiving short shrift.⁴³ That was of course the point to the committee. And equally a sign of the uphill battle they were facing with some former industrial partners who now had the opportunity to shop around with their research dollars, which had not been possible before. Feared loss of previous industry ties and federal dollars was very much a part of the development of biotechnology research from the point of view of the LGUs

⁴³ Letter from William Marshall to Al Wood, dated July 8, 1983. (Gainesville: University of Florida Archives).

-- a completely different situation from the private universities now clambering to get on board the federal and industry funding train.

Biotechnology, ultimately, was not a means for the LGUs to forge new unholy alliances with industry. As much as anything, it was a threat to many of those earlier alliances, forcing the LGUs to adapt to new industry demands for fear they might now go elsewhere. This threat would, indeed, force concessions and lead to new programs--joint ventures, biotechnology centers--and even to new industrial partners as the industries themselves changed and developed under the influence of the new technologies. But it was the opening of a flower already in bud, not the planting of a revolutionary seed.

It was quintessentially a triumph of the old order over the authentically new--the rural sociologists and green-(in the political sense, not the agricultural sense) revolutionaries who tried, and failed, to burst the reductionistic/production paradigm of biological science that agricultural research had established as its impregnable base.

If we are to continue in the mode of revolutionary metaphors then, biotechnology can be seen as the Napoleon that crushed the burgeoning revolution and re-established Empire--much to the relief of the previous aristocracy.

CHAPTER 7 NATURALIZING THE UNNATURAL

Few outside the academic community were likely in the Reagan years to complain in a sufficiently threatening way of increased ties between government research and private industry. The greatest threat perceived to the development and spread of the new biotechnologies was consumer fear.¹ It was this fear that seemed to most worry the land grant university research administrators and scientists in their attempt to move full force into this new "research for tomorrow." To allay and diffuse consumer fears, the agricultural establishment, with, of course, full industrial support, traded on the credibility it had gained throughout the years as consumer watchdog and protector of "purity" in food.²

Sheldon Krimsky and Roger Wrubel point out cogently the "struggle for control over the symbolic meaning of

¹ This lay behind much of the efforts to "educate" the general public as to the safety and "naturalness" of biotechnology. The spectacle of classic chefs boycotting genetically engineered tomatoes in the 1990s was proof of the validity of the administrators fears.

² USDA and the SAES had been involved in testing for product adulteration from the very outset. After the Pure Food and Drug Act of 1906, food and drug inspections by USDA/FDA served to create consumer confidence in this arm of the government.

biotechnology and about the process of mythmaking in science."³ They point to a series of myths/anti-myths, which this dissertation has demonstrated were fundamental in the thinking of the land grant university adoption of biotechnology and the resistance which it faced.

The issue of myth has been addressed often before in agriculture.⁴ It is unlikely that such a profoundly ancient and critical craft, so tied to religion in the early days, so associated with "values" and politics in our own, could escape fulfilling a mythic role. Biotechnology took its place in the rhetorical struggles.

The most important of these struggles over mythic territory for the initial success of biotechnology in agriculture was, of course, winning the debate over whether biotechnology produced "natural" vs. "unnatural" products. Krinsky and Wrubel have assessed this debate primarily on the basis of the biotechnology industry, especially small venture capital companies having initiated the mythmaking as

³ Krinsky and Wrubel, Agricultural Biotechnology and the Environment: Science, Policy and Social Issues (Urbana: University of Illinois Press, 1996). p. 215.

⁴ Leo Marx discusses the various earlier myths referenced in agricultural analysis, referring to Richard Hofstadter's "agrarian myth" or the Henry Nash Smith's "myth of the garden," both used as ideological weapons in the debate between rural and industrial ideologies and constituencies. (See: L. Marx, The Machine in the Garden: Technology and the Pastoral Ideal in America, p. 7-9). It is hardly necessary to go back therefore all the way to the Classical mythological figures of Persephone, Demeter and Ceres, to find fundamental links between mythological tropes and agriculture.

a way of carving out market opportunities against large chemical companies and at the same time as calming a jittery public.

Krimsky and Wrubel pointing again to industrial dominance, claim that "increased collaboration between university scientists and industry is eroding the value of the critical independent perspective of academic scientists on the assessment of new technologies. If that role is relinquished, biotechnology will become self-reifying."⁵ The difficulty with this analysis resides in its misinterpretation of the history of American agricultural research. It implies an artificial separation of industrial concerns from academic interests. Biotechnology, in the basic and applied "hard" science academic community, was essentially self-reifying from the start. It was part of the basic reification of industrial corporate capitalism, scientification and "progress" mythology that had been an undercurrent from the inception of the agricultural research system. It represented the cornucopia view of Eden.

Perceptions of even the possibility of an independent university scientist reflect a lack of acknowledgement of the cultural uniformity agriculture's myths imposed upon the research community from its inception and shows a faith in other myths, including the myth of scientific objectivity,

⁵ Krimsky and Wrubel, Agricultural Biotechnology and the Environment, 1996. p. 231.

especially that of the academic researcher set against the profitmongering industrialist.

In addition, the independent perspective they cite was actually an artificial enthronement of another discipline bound up by these same myths, the discipline of rural sociology, one of the key academic defenders of the counter paradigm against which production scientists used biotechnology as a vaccine. This other paradigm embraced the holistic Eden, one that was farmer oriented rather than production driven, "organic" rather than driven by agricultural chemistry.

Rural sociologists and industrial techno-critics have seen industrial capitalism as an outside corrupter rather than an intrinsic partner in the very concepts of agricultural productivity and agricultural science. They have seen themselves as independent critics, more objective in dealing with overall analyses of biotechnology. But their objectivity can be construed as a subjective defense of their own constituency--small farmers and small town America and its supporters, and they waged their defense as strongly as production agriculturalists defended their constituencies.

Krimsky argues that there were paths not taken toward increased productivity, ones that were subverted by industrial chemical designs. He accepts the rhetorical arguments of "alternative agriculture" with as much faith as

many of the agricultural scientists embraced biotechnology. Granted the importance of mythmaking and the very myths that Krimsky and Wrubel so accurately describe, these authors ignore how deeply myths were part of the science itself and thus such an analysis creates an external industrial straw man as well as formerly independent, now seduced, academics. Neither, however, existed in reality.

Members of the NASULGC, as shown in the previous chapters, were intimately tied to industry from the beginning. The goals of industry and the goals of academic science (supposedly geared toward the farmer) since the days before industry/university contracts became the norm have changed very little. In the modern era, the two groups have begun to cooperate more closely, but not to different ends. In earlier eras industry would often complain that there was no point in doing "varietal" research because subsidized plant breeding at government institutions was not so much doing it for them, as making it impossible for them to compete.⁶ Increasing yield/decreasing costs were inherently production concepts, whether fostered by industry for profit, or by the university for "public weal." Both relied on the same technologies and research paradigm, whichever

⁶ Kloppenburg, First the Seed, 1988, p. 12-13, points out how "public breeding programs constituted an institutional barrier to the systemic penetration of plant breeding and seed marketing by entrepreneurial capital. . . . the development of "finished" varieties by public agencies meant that the products of public research competed directly with those of private breeders."

worked. And both resulted in a greater industrialization of agriculture at the expense of many traditional small farmers and rural communities.

When biotechnology "emerged" it seemed the unique solution to the problems of agriculture (and, as importantly, agricultural science) to men like Al Wood and John Fulkerson. The research establishment embraced it as the means to the same general end as industry, but for different payoffs -- career success, peer and societal respect, financial rewards from grants and contracts, all through participation in the "myth" of an American Eden. It was an America free from want and fully productive, the breadbasket of the world. These payoffs were the reason for fighting over the symbolic ground of natural/unnatural. They were the reason why the embrace of biotechnology was not merely an industrial project but a program at the heart of the interests of the independent academics Krimsky and Wrubel seem to elevate to an almost hallowed status.

The first and foremost direct opponent was, of course, Jeremy Rifkin, whose attack on the very existence of genetic engineering resonated profoundly with a public ever-more fearful of a science outside of their understanding and control. But it was to be followed by more coherent threats from alternative agricultures that attempted to turn USDA's own productivity rhetoric against them, and to resurrect the agrarian values of the "family farm" and the very concept of

"food" as weapons against the designated institutional custodian of both.

As the technologies themselves were portrayed as "emerging" rather than being invented or created, they achieved status as facts of the natural world that were uncovered and therefore must be acknowledged and dealt with. They were coming forth, like it or not. This rhetoric created the demand for a positive response by experts to advise and discover in a reasoned and unemotive fashion society's "necessary" response to these inevitably emerging facts of life. Thus it was critically important to create the rhetorical aura of expertise at the same time as this requisite for experts was demonstrated. USDA and the LGUs not only had to enter biotechnology, they had to convince others of their intellectual prestige in coping with it. Part of the NASULGC mission was to provide a battery of information to demonstrate LGU competence and initiative in this new domain.

Ultimately, in the same fashion as the technologies "emerged" from the background and became self-evident, so too did the requisite policies outlined in the various NASULGC reports appear to emerge from the reasoned and factual status of the tables and surveys presented. To deny the constructed reports was to deny the evidence of not only experts, but the reader's own reasoned judgement in the face of such unproblematically and objectively displayed and

debated "truths." The discourse of Western scientism played itself out through the debate.

Biotechnology, through the process of a discourse that normalized and naturalized the power and productions of science, undercut conceivable challenges to that power and liminalized all protest and protesters, damning them to the chaotic, emotionally distraught hell of the "unscientific." More importantly, it forced them to uphold the illogical position of condemning "Nature" as "unnatural." At the same time, it put its critics in the selfish position of denying their fellow mortals the fruits of controlling that same nature.

But to do this to confound their critics, it was necessary for the promoters of agricultural biotechnology to deal with one prominent contradiction: if agricultural biotechnology was a self-evident and emergent scientific good, how could it be underfunded and under attack? Something was wrong that must be righted. This problem was evident in the clearly apparent confusion of the committee when trying to address this issue.

If Biotechnology was really new, then it was indeed an unknown variable, as Rifkin and fellow critics claimed, and therefore potentially dangerous and subsequently amenable to the charge of "unnatural" with all of the legal and ethical ramifications such charges might incur. If it was truly new and invented rather than "emerging" there was no inherent

necessity that society accept its existence and biotechnology could be legitimately stopped if so desired.

On the other hand, if biotechnology was not new, then why should government and society look to it for the nearly unlimited benefits previously impossible being promised by its proponents? Why provide massive increases in funding to achieve these new possibilities if they weren't really new at all?'

The solution to this problem required the creation of an obvious but soon to be almost universally accepted contradiction. Agricultural biotechnology was simultaneously defined by the document as both new and not new, man-made and yet not unnatural. Repeatedly the texts played out these contradictions and attempts to uncreate them. On the one hand it was claimed that "Biotechnologies in the form of improved plants and animals have been the centerpiece of agriculture production for at least 80 years and in a rudimentary way for the past several thousand years."⁶ But the same document asserted that "The new biotechnologies are those. . . developed through advances in molecular genetics.

⁷ An increase in both funding and social good (the latter being dependent on the former) was always the declared goal of the committee's various publications, not to mention the twin goals of the agricultural research establishment in general.

⁸ Committee on Biotechnology, Emerging Biotechnologies, Progress Report II, p.1.

. . ."⁹ There was a concerted redefinition of "nature" and the normalization of the "unnatural." The main gene transfer system for plants even in these new biotechnologies, for example, was a "natural vector for introducing genes into plant genomes."¹⁰ Micro-organism are involved as "traditional biotechnologies" but also as the new "microfactories."¹¹ Indeed, biotechnology was touted as a creator of "natural pesticides"¹² and the micro-organisms, far from being unnatural hazards, could be utilized "to eliminate already existent hazardous situations."¹³

As suggested, these moves liminalized protest by defining away its authoritative stance. With regards to a lawsuit against the new technology by Rifkin, the committee stated: "Ultimately, a decision must be reached as to whether or not DNA fits the legal definition of a chemical substance and whether or not genetic engineering changes DNA

⁹ Committee on Biotechnology, Emerging Biotechnologies, Progress Report II, p.21.

¹⁰ Committee on Biotechnology, Emerging Biotechnologies, Progress Report II, p. 23.

¹¹ Committee on Biotechnology, Emerging Biotechnologies, Progress Report II, p. 24. (This certainly sounds like the ultimate in industrialization of biology!).

¹² Committee on Biotechnology, Emerging Biotechnologies, Progress Report II, p. 25.

¹³ Committee on Biotechnology, Emerging Biotechnologies, Progress Report II, p. 25.

to a new chemical substance."¹⁴

These concepts were remarkably easily adopted by the academic and government communities as a whole, by supporters and critics alike. The subtitle of Plants, Power and Profit, a relatively critical look at the development of biotechnology in agriculture, looked at "Social, Economic, and Ethical Consequences of the New Biotechnologies." It spoke not only of emerging biotechnology, but also of emerging trends and issues. It demonstrated how quickly the definitional wars of biotechnology (for good or ill) were won by the agricultural establishment that had such a profound stake in creating continuity and evolution in biotechnology.¹⁵

But winning the battle of new and old biotechnologies was not the only rhetorical triumph. In an ultimate affirmation of the need for biotechnology, the final appeal to the court of public opinion was consistently presented, not just in Emerging Biotechnologies, but in nearly every debate of the post-Reagan era in which high technology and progress was an issue in general public or Congressional

¹⁴ Committee on Biotechnology, Emerging Biotechnologies, Progress Report II, p.73. In other words: old DNA transformed to new DNA, in their view, was still DNA -- new and old in the same breath. Innovation and tradition. All "natural" ingredients, despite "new" combinations. To a large extent this was the playing out of the central dogma, that DNA chemistry was life.

¹⁵ Busch and Lacy, Plants, Power and Profit, 1991. pp. 1-30.

forum. It was an appeal to American patriotism, especially in its new-minted guise of national "competitiveness."

Instead of "my country right or wrong" one now heard "my country, natural or unnatural" as the paean for the "new" biotechnologies. All unresolved contradictions were silenced by appeal to a "higher" unity: "It is essential that all necessary steps are taken . . . to insure that these new biotechnologies are developed and adopted. American agriculture still has a competitive advantage. It must be retained and exploited."¹⁶ It thus became the duty of every American to embrace biotechnology.¹⁷

It is interesting to note how the phrase: "The Silver Bullet" became associated with the 1983 report as its common-place nickname among the agricultural administrative community. The image of the destruction of the lycanthrope¹⁸ coupled to the heroism of the "Lone Ranger,"¹⁹ spoke volumes

¹⁶ Committee on Biotechnology, Emerging Biotechnologies, Progress Report II, p. 21.

¹⁷ Similar arguments were raised in the development of nuclear power after WWII. This becomes even more interesting if one makes the analogy that biotechnology is agriculture's chief weapon, and agriculture one of the main fronts in the war for world market domination that seems to have replaced the Cold War.

¹⁸ A werewolf (at least via Hollywood films) was well known to be destroyed by silver bullets. Was this a natural biotechnology experiment gone wrong? Or more likely was it a vision of Jeremy Rifkin, the newly liminalized and hence "abnormal" protester against the natural and self-evident benefits of agricultural biotechnology?

¹⁹ In the early television series, the Lone Ranger would help out the Western community and then, without

as to the self-recognition of the embattled and noble spirit of the document as a defender of emerging science against superstitious fears of progress.

Yet the issue of natural or unnatural would not go away. Much about the very nature of the agricultural biotechnologies (old AND new) was a disruption of "natural" sexual reproduction for the purposes of control, a tremendous problematic to modern feminist critics of science.²⁰ In Emerging Biotechnologies much of the language reflects these biases toward manipulation and control. Modern scientists were seeking to remove barriers imposed by the "necessity of sexual reproduction."²¹

In the new forms of reproduction, ova were "recovered" from the female animal for more appropriate and beneficial treatment. Hormones were classified with "other chemicals"; "normal sexual reproduction" was "bypassed;" genes were "isolated and subjected"; recovered ova were transformed and implanted into "normal utera" not female individuals.²²

The reductionism involved in genes being the

waiting for thanks, would vanish, leaving behind a silver bullet as evidence of his having been and gone.

²⁰ Donna Haraway is especially troubled by much of the possible ramifications of biotechnology to human sexuality and gender issues.

²¹ Committee on Biotechnology, Emerging Biotechnologies, Progress Report II, p. 1.

²² Committee on Biotechnology, Emerging Biotechnologies, Progress Report II, p. 24.

controllers and definers of traits and the body as object creates an anti-holistic destruction of the individual who became instead a package of desirable or undesirable genes: ("single gene genetics") rather than the more heterogeneous approach of "whole-organism genetics."²³ Reproduction was transferred to these new scientist "breeders" and away from the actual reproducers.²⁴

As part of the self-proclaimed goals of biotechnology, especially commercial ventures, life itself, as genes and "sexual varieties" was profitized, and made a patentable product in the ultimate power-seizing of what some feminist and humanist critics saw as a patriarchal or industrial system intent on deconstructing the resistance of the individual by defining away its very existence into bags of genes or transforming female animals into passive receptacles for the growth and development of genes.

With such ubiquitous technological domination, the ancillary patriarchal claims of patriotism and "competitiveness" need hardly be addressed: "Our nation's competitive advantage relative to other nations will be retained to the extent biotechnology research is fully

²³ Committee on Biotechnology, Emerging Biotechnologies, Progress Report II, p. 22.

²⁴ Donna Haraway in Simians, Cyborgs and Women: The Reinvention of Nature (New York: Routledge, Chapman and Hall, 1991) p. 43-68, is especially troubled by this translation of biology and sex into human engineering terms and profit motives.

exploited by us."²⁵ Whatever the long term social ramifications of such a reductionism of nature to DNA and sex to genes, the benefits to agricultural biotechnology at claiming "nature" on its side were immense. It gained the new biotechnologies a legitimacy they might have otherwise lacked by removing one of the key rhetorical points their attackers wielded against them.

As Krinsky and Wrubel have pointed out, the response of USDA and its allies could indeed be seen as part of a continuing effort that had begun years before the new biotechnologies, when convincing consumers of food safety with regard to pesticides seemed a critical necessity after "Silent Spring." In the current instance, USDA would reassert its claims that it had been the protector of food purity and the ward against chemical adulteration since the beginning of federal regulations. Biotechnology would trot out its FDA "hat" to convince the public of its general good will and trustworthiness.

Here is a contradiction between those who see regulation as an onus upon business and those who see it merely as a protection "of" certain businesses. It provides a problematic for seeing university demands for more structure and controls as inimical to biotechnology or industrial relationships. Regulation, especially self-

²⁵ Committee on Biotechnology, Emerging Biotechnologies, Progress Report II, p. 20.

regulation can be seen as one of the key methods by which corporate capitalism developed: it both managed excess competition, eliminated marginal players who could bring down unnecessary social wrath and provided public assurance of corporate good will. The entire progressive agenda, formerly interpreted as altruistic is now frequently interpreted as simply industry smoothing out its own path and protecting itself from even more onerous public interference. The Pure Food and Drug Acts and meat inspection laws that gained USDA such consumer faith over the years were as much a service to industry as a consumer benefit -- something that was often forgotten when calculating the agricultural research establishment's long history of industrial ties and cooperation.²⁶ In addition, professionalization in the practice and use of biotechnology demanded the development of regulatory structures in order to define its reach and especially to exclude outsiders.

Ultimately, the battle between natural and unnatural would move to a confrontation of industrial farming methods and goals against so-called "natural foods." The developing threat of "organic gardening" was represented most

²⁶ P. Starr, The Social Transformation of American Medicine, 1982, p. 130-132, pointed out how the Pure Food and Drug Act of 1906 primarily enabled the developing drug industry to crowd out small competitors and enhanced the power of the medical profession at the same as doing little to protect the public against actual fraud or damage. It was another example of the rise of corporate capitalist regulation.

powerfully by the Rodale Press, (publishers of the magazine Organic Gardening, whose Congressional might by 1984 was such as to be a key influence in bills attempting to force USDA into greater pursuit of "alternative" agriculture. It was a battle critical to biotechnology and industrial agriculture as a whole that "organic" not gain the common credence of natural, for that would force the "unnatural" label onto standard practices using chemicals, much less the new genetic engineering techniques.²⁷

If anywhere, it is here that Krinsky's independent academics take the floor, representing their own constituencies of rural sociologist, environmentalists, animal rights, and "Pure Food Advocates," each of which had close financial ties and shared "myths" with the academics who represented them. Adversarial politics, not the search for truth had come to be a key component of agricultural legislation by this period, with agricultural policy not a concordance of debated ideas rationally leading to conclusion, but a patchwork response to irreconcilable demands temporarily compromised, with each side determined

²⁷ G. E. Brown Jr., Subcommittee Chairman of the House Subcommittee of Department Operations was a staunch friend of "organic farming." In his "Opening Statement," p. 1-2., In: Agricultural Productivity Act of 1983, Hearing: First Session on H. R. 2714, August 3, Serial No. 98-50 (Washington, D.C.: U. S. Government Printing Office, 1984), he declares: there is a need "both to find better ways to control plant diseases and to improve the economics of farming. . . . Organic farming is beginning to be seen as a serious alternative, indeed as the only alternative presently available."

to fight on in the next legislative session.²⁸

Krimsky and Wrubel are completely right in seeing the debate hovering about myths of natural v. unnatural; greater v. less control; greater v. less biodiversity; friendly v. unfriendly to the environment. If it was industry v. the common man, then it was also the "hard" biological sciences v. the "soft," not as hired guns on either side, but rather as cultural fellow travellers.

It was reflective of a schism in society that was incorporated into the "new" USDA structure post-1960s as internally conflicting as that of the states vs. federal camps that had created research conflict from the start. The two paradigms of science and scientific agriculture v. the farmer and an "organic" Eden were forced into conflict. As historically had always happened, "scientific farming" and progress asserted its power. Biotechnology proved a trump card.

With amazing dexterity, biotechnology itself became naturalized and was presented as the choice in alternative agricultures. It is in this area that the political power of biotechnology to be all things to all people made such sense in the agricultural system. As pointed out in modern policy

²⁸ For discussions of this change in agricultural politicking see: Bonnen et al, Further Observations on the Changing Nature of National Agricultural Policy Decision Processes, 1946-1995, 1996. and D. F. Hadwiger and W. P. Browne, The New Politics of Food (Toronto: Lexington Books, 1978).

analyses, agriculture was ever more subservient to ever more diverse groups of splintered interests, all of whom had clout in forming farm legislation.²⁹ There were no more "glory" days when the federal government, the agricultural establishment and its associated commodity lobbies were the only players in town.

Biotechnology's claimed ability to produce "natural pesticides" and "biological" solutions to agricultural problems of fertilization, disease, and pollution saved the day for the nascent industry. The American Farm Bureau would testify to that effect to the disgust of its more radical anti-production oriented sister groups.³⁰

Rhetorically speaking, biotechnology represented agriculture's version of trickle-down, supply-side economics. It would claim to increase economic prosperity (not to mention national competitiveness) in ways that were not only safe, but optimal for every socio-economic level, despite the apparent appearance of being tailored for large-scale industry and big business alone. Since it was "emerging," and represented progress, how could it be

²⁹ See: Bonnen et al, Further Observations on the Changing Nature of National Agricultural Policy Decision Processes, 1946-1995, 1996. and Hadwiger, D. F. and Browne, W. P. The New Politics of Food (Toronto: Lexington Books, 1978).

³⁰ B. Hawley, Testimony to the United States Senate, Agricultural Productivity Act of 1983, Hearing: Second Session on S. 1128, June 1984. pp. 72-77. Hawley was Assistant Director, Washington Office, of the American Farm Bureau Federation.

denied?

And this was not self-serving hypocrisy on the part of biotechnology's advocates. The progress inherent in this quantum leap in agricultural science seemed truly to have inherent within it the answer to all the problems for which it was touted. This promise, if nothing else, can help explain the astonished disbelief of those production researchers and administrators who just could not understand the public fears, nor the resistance of their academic peers in the social science side of farming to the new technology that would so obviously be to the benefit of all.

It explains their absolute conviction that "education" was the key to changing their minds, and that an enlightened public would automatically abandon all their fears and embrace the new progress and the new science. It was the conviction of zealots who knew that all that was required was a missionary movement for the unconverted or false worshippers to realize the true righteousness of their ideology and to change sides.

CHAPTER 8
BACK TO THE FUTURE: THE "TRIUMPH" OF BIOTECHNOLOGY,
"PROGRESS," AND THE LAND GRANT SYSTEM

The "triumph" of biotechnology is of course by now an old story. It transformed the way much, if not most, of biological science was done from technological uniformity via corporate kits to common jargon and procedures. It transformed grants and funding patterns so that the majority of the non-formula related new monies were almost exclusively for biotechnology related research. But for the agricultural research system and the LGUs, it provided something more. It provided salvation not only for the endangered paradigm, but also for the institutions it had fostered. It is no wonder, then, that biotechnology institutes developed at almost every major LGU.¹

USDA retained control of the release of transgenic plants and control of most biological safety issues related to agriculture via the Animal and Plant Health Inspection

¹ Curry, J. and Kenney, M. "Land-Grant University Relationships in Biotechnology: A Comparison with the Non-Land Grant Research Universities." Rural Sociology 55(1): 44-57, 1990. contains a survey of biotechnology faculty in the LGUs which found that nearly 70% of them worked in universities with biotechnology centers or institutes.

Service (APHIS) under the Plant Pest Act.² Equally, if not more importantly, FDA regulations to deal with genetically engineered foods were developed such that "the key factors in reviewing safety concerns should be the characteristics of the food product rather than the fact that new methods are used."³ In short, the debate over natural vs. unnatural was effectively won. New genes were deemed no different than any other "additive." DNA was indeed just another chemical already present in the plant and USDA was able to regulate release of transgenic plants as they would deal with any other potential pest research.

Biotechnology approaches spread to recalcitrant species, including forest species. Biotechnology, taught at the LGUs, spread to Third World countries. Biotechnology followed a standard pattern of professionalization typical to the development of new disciplines and research areas -- new journals became available and changes in acceptance patterns for submission in old journals occurred. It would

² Committee on Biotechnology, Emerging Biotechnologies: Issues and Policies, XI (1992), pp. 37-38. Regulations set forth in 1987 (7 CFR 340) gave APHIS control over genetically engineered plants containing DNA from organisms which are plant pests or which APHIS has reason to believe are plant pests. This has been expanded and interpreted to include almost all transgenic plant field testing.

³ Committee on Biotechnology, Emerging Biotechnologies: Issues and Policies, XI, (1992) pp. 39-41. The Food and Drug Administration, which began under USDA, is currently a regulatory agency under the Department of Health, Education and Welfare (HEW). They work closely with USDA and appear to be highly in tune with USDA/SAES interests in labelling and regulation. USDA retains nutritional guideline authority.

become requisite to use the new biotechnologies for most laboratory studies in physiology and breeding. Use of biotechnology methods became as requisite to publication in a number of journals as statistics had been to an earlier era.

The biological/industrial complex further blossomed as agriculturalists hoped and social scientists feared, creating increased moves toward the industrialization of all aspects of agricultural production.⁴ The transformation of the rhetoric was completed--sciences "emerged," biotechnologies were seen as new or old and USDA had all but won the "natural" debate.

Biotechnology products were on the fast track to the market and disease and insect resistant new plant varieties were coming on line.⁵ Safety issues were muted as Rifkin shifted his sights to other game than genetic engineering.⁶ Biotechnology information issues percolated to "extension" to an even greater extent, serving as both an advanced warning system, but also as an early diffuser of potential controversies.

⁴ J. Curry and M. Kenney, "Land-Grant University/Industry Relationships in Biotechnology: A Comparison with the Non-Land Grant Research Universities." Rural Sociology 55(1): 44-57, 1990.

⁵ Weiss, Washington Post, October 8, 1996 A1, A4.

⁶ Rifkin's current book is entitled Entropy and is an examination/attack on modern technology and industrialization as a whole, not restricted to biotechnology.

Many of the clientele groups repositioned without rhetorical consequence; industrial partnerships flourished and commodity groups pushed for the use of biotechnology to improve their markets and develop new products from old produce.⁷ National security and competitiveness issues gave high technologies high priority across the board and biotechnology found itself in the reflected honor of being among other resources for the future.⁸ Many of the most significant issues raised by the "Silver Bullet," while not resolved, shifted emphasis considerably. Issues of industrial intercalation into the universities translated more fully into questions of law rather than ethics.⁹

⁷ A USDA focus, as witnessed by the 1992 Yearbook title: New Crops, New Uses, New Markets, was on helping traditional crop producers find new uses for their surpluses, especially pushed by grain commodity organizations demanding increased research into biofuels (p. 199-230) and using biotechnology to develop new uses for starch (p. 147-153) and vegetable oils (p. 154-158).

⁸ International competition in biotechnology and other high tech ventures such as computer technology became fierce, with European and Asian governments (especially Germany and Japan) providing subsidies to their developing industries (USDA, New Crops, New Uses, New Markets, 1992. p.22), so much so that time and again the U.S. government was called upon to match its efforts to theirs by internal industry and agricultural lobbyists. George Kozmewsky outlined the prospects and needs in his presentation: "Commercializing Biotechnology Resources: Competition and Cooperation in Global Markets." p. 209-221, In: Vasil, I. (ed) Biotechnology: Perspectives, Policies and Issues (Gainesville: University of Florida Press, 1988). Key to competitiveness in his view were research support from the federal government and regulatory relief.

⁹ The Committee on Biotechnology reports after the mid 1980s began to treat less and less of ethical issues and more and more on industry cooperation, patent issues and

Land Grant Universities became more competitive, in many cases outcompeting their private sisters for new grant funds and industrial investment. Personnel important in the early development of biotechnology in both university and industry were enshrined in positions of power and prestige.¹⁰ Mythologies were re-invigorated. Science proceeded.

Despite the general acceptance of the concept of "emerging" and the concept of "new" biotechnologies, by university and government administrators, critics and the public, agricultural biotechnology could not ever truly be new, just as it did not emerge. It was the concerted product of a research program that would inevitably mold the new technology into the patterns of its own myths, ideologies and goals. It vented steam from a system under pressure by making possible once more the dreams that an alternative paradigm seemed intent upon smashing.

regulatory policies.

¹⁰ Curt Hannah became head of the Interdisciplinary Center for Biotechnology Research (ICBR) formed at the University of Florida. Indra Vasil was given expanded laboratory facilities and remains a sought after speaker and lecturer on the world biotechnology circuit, especially with his successful production of whole plants regenerated from wheat protoplasts. Roger Beachy is currently head of the Division of Plant Biology at the prestigious Scripps Research Institute of Plant Biology, La Jolla, CA and heads FDA task forces on evaluating genetically engineered plants. The majority of the other scientists mentioned in this dissertation (if not dead or retired) have moved up the career ladder in industry and academia with at least equal but most often greater success than their non-biotechnology oriented peers.

The rhetoric of progress could be re-enshrined with new possibilities and "answers" to the counter-paradigm nay-sayers. Answers that, critically important, would convince the general public and the federal government at large. "Scientific" agriculture was back in business at the local scientist and state level. And, although the institutional capacities of USDA would still be questioned, the distancing machinery of the SAES from many of the Federal issues and non-production programs protected it from much of the political fray.¹¹

For the scientists in the laboratories who embraced biotechnology, the benefits were most obvious: computers and kits and a common molecular biology culture would link the agricultural biotechnology community to Nobel Prize winners in medicine/chemistry and to scientists publishing in the most prestigious of journals. Much of the fears concerning loss of prestige of an earlier era could be muted in the minds of "biotechnologists" who could retain a dual citizenship as scientists and agricultural researchers (basic savants in an applied field) much as the original goals of an Atwater and True would have made them.

¹¹ According to Bonnen et al, "Further Observations on the Changing Nature of National Agricultural Policy Decision Processes, 1946-1995." Agricultural History 70(2):130-151, 1996., USDA became much more subject to conflicting policy demands as its clientele expanded hugely in the post-WWII era. The SAES, more reliant on individual state interests and local commodity organizations, had less contact with many of the conflicting groups that fractured USDA policy-making abilities.

Genome projects co-sponsored by USDA with competing agencies such as NIH and DOE would seal biological unity under the master molecule rubric, but more importantly would ally agricultural scientists with the major streams of biological reductionism and big biology. Biotechnology embodied the production metaphor of basic research leading to applied goods that came straight from the 1860s through Vanevar Bush to the present. It would provide almost every one of Hadwiger's key reasons "Why Scientists Work" from income to peer group esteem to funding and especially to ideology.¹²

Connections to computer links and overlaps in universal data bases drove home the unity of biology in a powerful way. The sense that they were researchers performing in an agricultural backwater all but disappeared among those working in biotechnology and genetic engineering. A moment on the Web and the same gene sequence was recognized on a computer search in Drosophila, humans, nematodes, bacteria, Arabidopsis, and the lowly corn variety the lab was working on. This provided agricultural biotechnology researchers with not just a greater sense of the unity of biology, but also of their own unity with the biological profession at its cutting edge.¹³

¹² Hadwiger, The Politics of Agricultural Research, 1982. pp. 51-67.

¹³ Typically biotechnology researchers felt somewhat isolated from their non-biotechnology peers, as revealed in

To non-biotechnologists, the situation, though not so obviously favorable was still acceptable, in that the rhetoric of research administrators such as Al Wood and others had been most careful to protect general agricultural research from loss of its formula funding, using the plea for basic research in other areas as necessary "support" for biotechnology both in initial discovery and ultimate development.¹⁴ Overall, however, the greatest boon was of necessity intangible. By protecting the production-oriented paradigm, nearly all traditional research could be protected under the biotechnology umbrella. Where there were problems with approaches, biotechnology promised the safety patches.

As mentioned in the introduction, biotechnology re-invigorated the land-grant university research system. It provided new monies and new institutions. Not only that, the industrial connections proved so successful that the Biotechnology Committee would, in its report on Industry-University Relationships, pointed out the desirability of working to "develop an ongoing input from industry on the research agenda in biotechnology in the land-grant

the survey done by Curry and Kenney (1990).

¹⁴ Although government agricultural research funding decreased at the same time biotechnology funding increased at the LGUs, it can be argued, given the nature of these institutions and the general decrease in research funding across the board, the overhead, graduate students, potentially shared equipment, office personnel etc. that biotechnology generated was a net benefit to all, despite jealousy that individual funding differences was apt to cause.

universities."¹⁵

Furthermore they would work to "develop a greater commitment by industry . . . for support of federal appropriations for biotechnology research in universities. Early involvement in agenda-setting for university research will create ownership on the part of industry for the outcomes, which they should be more willing to support politically."¹⁶

So intimate a relationship was envisioned that it was even recommended that industry provide "prescriptive input . . . on graduate training in biotechnology, perhaps extending to shared sponsorship and training of graduate students and post-doctoral fellows."¹⁷ So much for the niceties of academic freedom--where one of the most important functions of the university and professional community--that of educating its replacements--was recommended to be "industrialized" along with the research itself.

The fears of the Buschs' and the Lacys' appeared to be coming to pass, but, as I have argued, not primarily from the unique aspects of biotechnology but rather from the

¹⁵ Committee on Biotechnology, Emerging Biotechnologies, XI, 1992. p. 6.

¹⁶ Committee on Biotechnology, Emerging Biotechnologies, XI, 1992. p. 7.

¹⁷ Committee on Biotechnology, Emerging Biotechnologies, XI, 1992. p.7.

internal evolution of land-grant agriculture towards industrialization from its inception. Biotechnology made the transition easier and more rapid but it was not a shotgun wedding driven by its unique demands by any means. They even arranged to do their own catering. The plan was for "CSRS staff and members of the subcommittee" to act as the negotiating agency with "senior industry representatives to address the agenda described above."¹⁸

But if anything more were needed to prove the non-revolutionary nature of biotechnology, the return of "crisis" to the debate in the 1990s in Congress provided a most telling example.¹⁹ "Alternative Agricultures" returned as the latest and most invigorated salvo on the side of the competing paradigm of anti-production, anti-technology, preservation of the family farm. Congressional statutes were passed and hearings were held attempting to convince (or order if necessary) USDA to adopt such more friendly technologies.²⁰

USDA policy maven, SAES officials and representatives

¹⁸ Committee on Biotechnology, Emerging Biotechnologies, XI, 1992. p. 8.

¹⁹ Here I feel woefully like a bad director of cheap horror flicks who ressurects the monster in the last ten seconds of the movie to titillate the audience with both the promise of more bloodshed -- much less a sequel -- but most especially with the lack of finality in all endings.

²⁰ House Subcommittee on Agricultural Research, Conservation, Forestry and General Legislation, Setting Agricultural Research Priorities, October, 1993.

of the American Farm Bureau came forward, nodded their agreement to the basic principles of environmentally friendly agriculture and protection of rural values -- then pointed to biotechnology, once again, as the solution. Once again it would provide new products and new markets for overproduction (thereby removing the need to decrease production-oriented research) and would replace "dirty" technologies with clean in a "natural" manner. This would all occur at the same time as maintaining a competitive edge in a world economy.

The battle between competing world views was well recognized by its practitioners. The two paradigms were quite used to each other and possessed, at times, of a dry humor about their conflict, despite their frustration at inabilities to create an acceptable common ground. In October 1993, the House Subcommittee on Agricultural Research, Conservation, Forestry and General Legislation of the 103rd Congress held their first session on "Setting Agricultural Research Priorities." Neill Schaller, associate director of the Henry A. Wallace Institute for Alternative Agriculture summed up the dichotomy well by creating the characters: Sustainable Sam ("a voice of the sustainable agriculture movement") and Tommy Test-Tube ("that of the research system in general") to argue their conflicting agenda. One need merely quote portions of their opening statements:

Sam: Sustainable agriculture is spoken everywhere in the research system now. . . . We are impressed by all of this, and yet we are also very uneasy. We see a lot of window dressing. What you call sustainable ag research looks a lot to us like the same old reductionist, piecemeal stuff that makes for good reading in disciplinary journals and gets scientists promoted, but doesn't have much to do with sustainability. . . . We still hear some of you muttering in the halls that sustainable ag is no way for the world to feed itself. . . .

. . . . you still use the old band-aid approach of doing research on ways to correct environmental and other problems after they occur, rather than studying ways to prevent the problems in the first place. You get glassy-eyed about things like biotech, but forget to ask about their possible negative impacts on people and the environment. . . .

Tommy: Now, wait a minute. We have been researching sustainable ag a lot longer than you have been pushing it. Look at what we have done to boost profitable production of food and fiber in this country and how we have responded to concerns about environmental degradation and soil erosion. . . . Maybe we should be acting more than reacting, but you folks seem to have your minds made up on what is needed to get a sustainable agriculture, so why bother? You are still on your kick about reducing chemical inputs and going back to hoes and hard labor.²¹

Schaller then trotted out "Mediator Molly" to try and resolve the problems between the two, but her advice was primarily for the agricultural research system to "listen" and to realize that sustainable agriculture was not necessarily "bad science" or "antiscience."

Shaller saw no need, apparently, to point out the

²¹ Testimony of Neil Schaller to the House Subcommittee on Agricultural Research, Conservation, Forestry and General Legislation of the 103rd Congress held their first session on "Setting Agricultural Research Priorities," October, 1993. p. 87.

deeper incompatibilities between the "Sam" and "Tommy" which are, as I hope I have shown in this dissertation, far deeper than notions of science v. non-science. It reflects their allegiance as well to the two sides of Eden still at war. Mediator Molly's chances of success are as good as if she attempted to "mediate" between Galileo and the Pope on the "truth" of the Copernican system.

In this same document, research funding was listed in an appendix for both ARS and CSRS. Here the forces affecting the two research institutional systems in the modern world become ever more apparent. USDA, subject to increasing pressure by these "alternative agriculture" groups has expanded its water quality and food safety funding levels dramatically. But, driven by market demands and the old industrial paradigm, the majority of new monies were going into "New Uses" for food and fiber crops--absorbing production increases rather than curbing them. No wonder "Sustainable Sam" was suspicious.²²

As for biotechnology, ARS maintained a substantial commitment to Animal Germplasm/Genome, but nowhere near the commitment to new technology shown by the CSRS, which, tied to the SAES has made its major funding increases in "Biotechnology" (17.6% of discretionary research funds)

²² Testimony of Neil Schaller to the House Subcommittee on Agricultural Research, Conservation, Forestry and General Legislation of the 103rd Congress, first session on "Setting Agricultural Research Priorities," October 1993. p.88.

despite also committing money to "Water Quality" (7.6%), "Nutrition Food." (5.8%) and "Aquaculture" (4.2%).²³

It seems, then, that biotechnology, as both a major program emphasis in the agricultural research system and as a rhetorical device in protection of the treasured reductionist/industrial paradigm, is here to stay. That is why it is such a contentious issue in both research and political debate. Its myths retain current ascendancy, its Eden of abundance for all captivates more strongly than its foe's vision of a rural paradise for the few. For now, at least, "progress" wins.

Atwater smiles.

²³ Appendix, the House Subcommittee on Agricultural Research, Conservation, Forestry and General Legislation of the 103rd Congress, first session on "Setting Agricultural Research Priorities," October, 1993.

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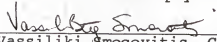
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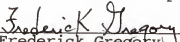
BIOGRAPHICAL SKETCH

Mark Steven Lesney was born on May 13, 1953, in Highland Park, Michigan, the son of Jerome J. Lesney and Stephanie R. Lesney (nee Obelnicki). He attended Wayne State University in Detroit and graduated in 1975 with a Bachelor of Science in biology. He attended Michigan State University in East Lansing from 1975 - 1980, receiving his Master of Science in botany and plant pathology in 1977, and his first Doctor of Philosophy degree in botany and plant pathology in 1980. From 1980 to 1984 he held several postdoctoral fellowships. From 1985 to 1992 he served as Assistant Professor in forest biotechnology at the University of Florida. In 1993 he initiated his Doctor of Philosophy degree program at the University of Florida in history of science, specializing in the history of modern biology and biotechnology.

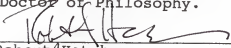
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Professor of History


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May, 1997

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